PROCEEDINGS

THIRD CAL/AVLABS SYMPOSIUM

Aerodynamics of Rotary Wing and V/STOL Aircraft

VOLUME III

Featured Speakers,
Panel Session On Recommendations
For Future Aerodynamic Research

18-20 June 1969
Buffalo, New York

CAL/AVLABS

SYMPOSIUM PROCEEDINGS

Aerodynamics of Rotary Wing and V/STOL Aircraft

Published in three Volumes as follows:

Volume I	Rotor/Propeller Aerodynamics Rotor Noise
Volume II	Wind Tunnel Testing New Concepts in Rotor Control
Volume III	Panel Session on Recommendations for Future Aerodynamic Research, Panel Summaries and Featured Speakers

FOREWORD

Within the past six years, the U. S. Army Aviation Materiel Laboratories (AVLABS) and the Cornell Aeronautical Laboratory, Inc. (CAL) have co-sponsored two symposia as a means for direct exchange of information regarding technical problems of relevance to Army aviation. The first, in 1963, addressed itself to dynamic load problems associated with helicopters and V/STOL aircraft while the second, in 1966, focused on aerodynamic problems associated with V/STOL aircraft. In the spirit of continuing this interchange among technical specialists from industry, the universities and government laboratories, we have planned the Third CAL/AVLABS Symposium. The theme of the symposium is "Aerodynamics of Rotary Wing and V/STOL Aircraft" and, at this time, we feel it is appropriate to cast a critical eye upon the technical progress that has been made in the years subsequent to the two previous symposia. It is important to identify those research problems wherein solutions have and can be reduced to engineering practice as well as to identify those areas wherein additional research and development is required.

Five technical sessions have been planned in an attempt to provide coherent presentations of current developments in rotor/propeller aerodynamics, rotor noise, wind-tunnel testing and new concepts in rotor control. In addition, a panel session has been organized to draw upon the experience of the helicopter and V/STOL industry in discussing current problems still needing solution, future problems that are beginning to show up on the horizon and, finally, to recommend areas for future aerodynamic research.

These proceedings consist of three volumes, the formal papers being presented in the first two. This format was chosen in view of the interrelationship between several sessions, in particular, those on rotor/propeller aerodynamics and rotor noise. Hence, at the risk of producing a slightly oversized volume, it was felt that in the interest of continuity, it would be more beneficial to include all the rotor aerodynamics and noise papers in Volume I. The papers on wind-tunnel testing and rotor control appear in Volume II. Volume III is devoted principally to the formal presentations of the panel session. Moreover, this session was tape recorded and an account of the verbal interchange between the panelists and the audience is also presented.

The Co-Chairmen want to acknowledge with thanks the cooperation of many people who have contributed to this symposium. Specifically, our thanks go to Colonel Eduardo Soler, Commanding Officer of the U. S. Army Aviation Materiel Laboratories, and Mr. Waldemar O. Breuhaus, Director of the Flight Dynamics Division of Cornell Aeronautical Laboratory, who opened this meeting; Mr. Alfred Gessow, Assistant Director of Research, Office of Advanced Research and Technology, NASA, whose keynote address,

contained in Volume III, served to set the tone for the technical sessions; and to our Banquet Speaker, Mr. A. Scott Crossfield, Division Vice President, Flight Research and Development, Eastern Airlines. We want to acknowledge the contributions of the five session chairmen — Barnes W. McCormick, Paul Yaggy, Robert G. Loewy, Mark Kelly and Dean C. Lauver — whose cooperation and counsel helped to shape the technical program. Our thanks also go to the authors and panelists for their cooperation in preparing manuscripts in a form that could be reproduced directly. This material was neither checked nor edited by CAL or AVLABS.

In conclusion, we wish to make special mention of the contributions of Mr. Patrick Cancro, Aeromechanics Division, AVLABS, whose efficient handling of countless administrative details guaranteed a smooth and coordinated effort between AVLABS and CAL.

SYMPOSIUM TECHNICAL CHAIRMEN

Alfred Ritter (CAL)

John E. Yeates (AVLABS)

CONTENTS

WELCOME BY HOST ORGANIZATIONS*

KEYNOTE ADDRESS

RECOMMENDATIONS FOR FUTURE AERODYNAMIC RESEARCH

*Transcribed from tape recorded speech.

AERODYNAMIC RESEARCH RELATED TO PROPELLER DRIVEN V/STOL AIRCRAFT
O.E. Michaelsen
Canadair Limited

PANEL DISCUSSION*
QUESTION AND ANSWER PERIOD

SOME PROBLEMS IN THE NUMERICAL SOLUTION OF VORTEX MOTION EQUATIONS**
J.P. Jones and M. Noak
University of Southampton, England

*Transcribed from tape recording.

**Messrs. Jones and Noak were scheduled to present a paper during Session I (Volume I of these Proceedings). Unfortunately, neither gentleman was able to attend the Symposium. The following paper is being published here because of the general interest of the subject matter even though the title differs from the originally scheduled presentation.

Waldemar O. Breuhaus
Director, Flight Dynamics Division
Cornell Aeronautical Laboratory, Inc.

Col. Eduardo Soler
Commanding Officer
U.S. Army Aviation Materiel Laboratories

Alfred Gessow
Assistant Director for Research
NASA Office of
Advanced Research and Technology

WELCOMING REMARKS

Waldemar O. Breuhaus
CORNELL AERONAUTICAL LABORATORY, INC.

and

Col. Eduardo Soler
U.S. ARMY AVIATION MATERIEL LABORATORIES

WELCOMING REMARKS
by
WALDEMAR O. BREUHAUS
Director, Flight Dynamics Division
Cornell Aeronautical Laboratory, Inc.

Gentlemen, I have the very brief but also very pleasant duty this morning to officially welcome you on behalf of the Laboratory. As Dr. Ritter said, this is the third, I believe tri-annual symposium on the subject of aerodynamics of V/STOL aircraft and rotor craft. I suppose that a numerologist might do things with the third tri-annual meeting. I think it's sufficient to say here that it's getting to be a custom, one which we think is very desirable, one which we hope is valuable to those of you who come from considerable distances to attend this meeting. I think that many of you have been here at previous meetings. We're trying to do it a little differently in some respects this year. As you may have noticed, we've arranged to have part of Niagara Falls turned off, so this is a little different from the type of tourist attraction which is normally here.

Now, as I say, my remarks will be quite brief because I don't want to usurp any of the territory of Colonel Soler or Mr. Gessow. One of the dangers when you're in a one, two, three order like this is that someone will give part of your speech before you and then you'll have to flounder a bit. I think the only remark that I would like to make is that we all recognize the complexities of the aerodynamics of V/STOL aircraft and rotor craft. This symposium gives an excellent opportunity to bring together specialists in this area to discuss new thoughts and old problems in a very old area -- low speed aerodynamics -- one which, in this day of space flight, is generally considered to be quite old hat. But I think we've all had the experience of finding that there are a great many unknowns and a great many uncertainties in this area, and I'm afraid also, unfortunately, some of the things in low speed aerodynamics which we knew well perhaps twenty years ago -- a few of these things may have been forgotten.

These symposia give a chance to bring back some of the old areas and to discuss some of the new discoveries, both in the formal meetings, such as we are having here and in the informal discussions and technical bull sessions that I know are going to be going on for the next three days.

So again I say welcome and turn the meeting over to Colonel Soler, Commanding Officer of the Army Aviation Materiel Laboratories.

WELCOMING REMARKS
by
COLONEL EDUARDO SOLER
Commanding Officer
U.S. Army Aviation Materiel Laboratories

Gentlemen, I consider it a very distinct pleasure and an honor to be here, to have the opportunity to welcome you to this third symposium, jointly sponsored by the Cornell Aeronautical Lab and our organization at Fort Eustis, Virginia, the U.S. Army Aviation Materiel Laboratories. I can assure you that our welcome is very cordial and sincere. We certainly appreciate your taking time off from your very busy schedules to come over and provide us the competence and also the prestige that you bring to this gathering. I trust that the administrative arrangements have been adequate and that all of you are very comfortably settled here in the local area, because, when I look at the ambitious agenda that you have set for yourselves, I think that you will need your rest inbetween your sessions here. However, this agenda apparently only shows a sequential, topical outline to be followed through a mechanism of individual presentations, individual papers, by what I could call a galaxie of stars in the field of aerodynamics of the rotary wing and the V/STOL field. However, it seems to me the total value to be derived from the symposium by you should not be the summation or the integration of the intelligence which is contained in these papers. I think the dynamics of a group like this one should permit us of the benefits to be derived more nearly factorial instead of additive.

I would hope that this symposium will accomplish a few things that might assist us in multiplying our benefits. I hope that this symposium will provide the forum for creative thinking on the part of the authors and also the part of the people who are present. I would also hope that this symposium will promote the exchange of all the knowledge among all of you who are here today, so that everybody again multiplies the benefits. During this symposium, I hope that there is going to be an environment of intellectual freedom of expression. In other words, nobody should be shy to express their views, ask

questions, participate in the discussions. And then again, more importantly, in my estimation, when you get a group of people together through the dynamism of group discussions, you should be able to obtain more than the summation of the knowledge and the intelligence of the people that are present. I think that if we do these things, when we leave Buffalo at the end of the three days we can say that we have really accomplished something.

Let me tell you a couple of things about my organization and my chain of command. A few changes have taken place in the last three years. As you know, AVLABS is a subordinate organization to the U.S. Army Aviation Materiel Command in St. Louis. There is a new commander at AVSCOM, in St. Louis, Major General John Norton. He is a very distinguished military figure in our country, a devoted citizen, and he's well known in industry and academic circles as well. He has brought vigor to our organization, dynamism to the system, and this thing has resulted in a revitalization of the Army Aviation program or, more specifically, the Army Aviation Research and Development Program. Since his arrival in St. Louis, General Norton has reorganized his headquarters to permit a more efficient management and direction of the functions and also to allow more attention to the management of total weapons systems, considering lift cycle management. The organizational scheme has proven to be so good that now many of the lateral commands parallel to AVSCOM are going to use this organization as a model. One step up the ladder, General Norton has a new boss, General Ferdinand J. Chesarek, the new Commanding General of the U.S. Army Aviation Materiel Command in Washington. He has already made his presence felt all through the chain of command. He has been to AVLABS, he's listened to our briefings. And I could spend much time telling you about this man or my impression of him. But if I were to use only one word to describe him, I would use that word called competence. He is a very competent individual. He's also an impressive leader and inspires great confidence in those who know him. Therefore, as I stand in front of you here welcoming you to this symposium this morning, I am filled with optimism and enthusiasm for what destiny may have in store for Army Aviation, or more specifically the Army Aviation R & D Program. I feel we have outstanding leadership in the Army and we have "policed up" the organization; and with

these two changes I think we should be able to accomplish bigger and greater things. But it seems to me we're only part of a team. You, gentlemen, are the other part of the team -- in my estimation, the more important part of the team. Without your help, I don't think we can ever get off the ground and become the Army Aviation all of us have dreams of. What I am trying to do today is solicit your assistance in helping us attain all the goals that should be attained for Army Aviation.

KEYNOTE ADDRESS
by
Alfred Gessow

Assistant Director for Research
NASA Office of Advanced Research and Technology

As some of you may know, I've been on the periphery of the helicopter and V/STOL field for the past ten years, and although I've maintained an active interest in it, I haven't had the time to follow the developments in it as closely as I would have liked. It was therefore with genuine pleasure that I accepted the invitation of John Yates and Al Ritter to give the keynote address at this conference in order to see them and many other old friends and to catch up with some of the advances in helicopter and V/STOL aerodynamics that have been made in recent years. Thus I can honestly say that I'm happy to be here.

I thought that it would be appropriate to this conference for me to share some views and opinions that I have regarding aerodynamic research on helicopter and V/STOL aircraft and where such research fits into the overall aerospace technology picture. Some of these views are old and reflect long-time prejudices; others have been acquired as a rider on the Washington merry-go-round.

In searching for a frame on which to tie the bits and pieces together, I thought that I might try the format used by the main speaker at my daughter's high school graduation exercises which took place last Thursday. His talk was entitled "Confessions of an Angry Grandfather." The speaker, an erudite and witty ex-Harvard professor with a new grandchild, gave his views on the morals and mores of contemporary society by prefacing each opinion with "I am angry with a society that allows such and such;" or "I am angry with my generation for permitting so and so;" or "I am angry with a small minority of college students who do thus and such." In this manner, he gave his views on racism, the war in Vietnam, student unrest, and other contemporary phenomena in a highly effective way which appealed to all the generations present in the auditorium.

I'm going to change the format somewhat. For one thing, I'm not a grandfather, either literally or with respect to the rotating-wing business. Secondly, the subject which brings us here today, though important to the

country and to us, does not have the emotional connotations that the sociological scene has. Therefore, I'm going to entitle my talk "Confessions of a Disturbed Middle-Ager."

What am I disturbed about?

To begin with, I am disturbed by the increasing distrust, suspicion and blame heaped on science and technology by increasingly large segments of our population. One effect, albeit a minor one, of this hostility is the significant percentage decrease in enrollment in science and engineering curricula at our colleges, curricula which traditionally attracted the brightest and in some ways the most idealistic of our youth. They, of all people, as well as the more intellectual members of our society, should realize that science and technology are not the cause of our problems, but rather represent the cures for our ills which cannot be brought about without the help of the physical scientist and engineer.

On the other hand, I am distrubed by the time it has taken many of our engineers and scientists to recognize and accept the responsibilities for their professional output, not only in the narrow technical sense, but in the social sense as well. I have in mind not only obvious things such as whether technology will result in increasing threats to the well being of our citizenry in the form of pollution or even more serious effects. I am referring to a sense of fiscal responsibility in order to insure that we are spending the taxpayer's money on worthwhile projects and that we are getting the maximum return on each dollar that we spend. We are all familiar with cost overruns, for example, but perhaps we shouldn't be so complacent about accepting them as a normal part of doing business. I feel strongly that the working-level engineer and scientist must share this responsibility with management, both within government and in industry.

I am disturbed by the short-sightedness exhibited by government and industry management in pouring so much of the R & D dollars into project-type development at the expense of research. In particular, I'm distrubed at the rush to get into flight development new designs and even new concepts before adequate theoretical, experimental and component research and development have taken

place. Unfortunately, we can cite all too many examples of the waste and inefficiency that such shortsightedness has brought about.

I am disturbed by the lack of appreciation of the nature of research by management who pay less attention to the quality and relevance of the research than to such management devices as PERT charts, milestone dates, and completion deadlines. Is it a case of empire building or is management more comfortable with such controls so that small, meaningful, basic research studies are downgraded in favor of large projects which involve a great deal of manpower, funds and large equipment? Or again, is it because the contribution of research to technology is not sufficiently appreciated? Why are there so few meetings of this kind as compared with those dealing with operations, design and project aspects of helicopters and other VTOL vehicles?

On the other hand, I am disturbed by some research people who continue to plow the same comfortable furrow long after the yield or usefulness of the crop warrants it. Research should not be synonymous with sinecurism. If the taxpayer is to support research, he has a right to expect good research for his money, i.e. research that is relevant as well as competently done.

I am disturbed by the very sharp and significant reduction in aeronautical research and graduate training in aeronautics that took place after Sputnik. We have been graduating Ph.D's in Aeronautical Engineering who are comfortable with plasma physics and quantum mechanics, but who don't know what CL_{max} is. Closer to home, how many universities in this country offer courses or conduct research in helicopters and V/STOL? Personally, I welcome the swinging back of the pendulum away from the almost total emphasis on space in the direction of aeronautics; it has a long way to go before a proper balance between aeronautics and space is achieved. I am disturbed, however, that in their understandable desire to do something on the immediate and real problems of air traffic control and aircraft noise, for example, not to mention our ever-increasing and all pervasive social problems, Congress and an apathetic public may inflict serious damage to our space program to which, like it or not, we and the rest of the world are committed.

When considering priorities for the distribution of funds, I am disturbed not only by the allocation given to aeronautics vs. space, or to research vs. advanced development projects, but also to the allocation between the helicopter and other V/STOL aircraft. Well before Sputnik, when the helicopter--though hardly into its teens--had already demonstrated its utility and practicality, sorely-needed research funds were diverted, unwisely in my opinion, from the helicopter to the flight development of a number of high disk loading VTOL aircraft of questionable utility. Although the distribution of R & D funds between low disk loading VTOL's is perhaps better today than it was then, I am very disturbed by the fact that although we know that we can make a barn door fly, there are those who insist we demonstrate it with little regard to the utility, efficiency, and operational aspects of the barn door as a flight vehicle. Although high disk-loading VTOL's someday will undoubtedly serve a useful role in the aeronautical picture, particularly when advances are made in lighter structures and power plants, it is my opinion that the bulk of the available funds and manpower should be placed on making the helicopter-type VTOL more efficient, quieter, faster, less costly, and easier to maintain.

After 25 years of production helicopters in this country, I am disturbed by our inability to either predict, understand, alleviate, or utilize the following:

1) Tail rotor--tail boom--main rotor interference effects; thus we have production and near production helicopters with inadequate directional control.

2) Aeroelastic, stability, control and vibration effects on nonarticulated rotors; thus we aren't ready to take advantage of the many desirable characteristics of such rotors.

3) Three-dimensional, non-steady stall and compressibility phenomena on rotor blades, including transient lift effects and flow hysteresis. Why can't we yet predict rotor thrust distributions in the stall condition, for example?

4) Effect of moderate (as opposed to very low) disk loadings on rotor hovering performance without using fudge factors.

5) Means for identifying the source and nature, as well as alleviating, engine and rotor noise.

6) Realistic blade load distributions under various flight conditions, including non-steady wake effects, atmospheric turbulence, and the like.

7) Jet flaps for control, propulsion and for stall avoidance.

8) Variable geometry rotors for improved efficiency over the speed range.

9) Improved airfoil sections with lower drag and moments, higher critical speeds, and good stall characteristics over the helicopter speed range.

10) Low speed and high speed flight problems (involving pilot displays and handling qualities) in adverse weather.

11) Reduction and isolation of vibratory loads and stresses.

This list obviously covers but a few of the items which need more work--each of you undoubtedly could come up with a better and longer list. The point that I am trying to make is that more--much more--of the relatively little funds that are available to helicopter and V/STOL research and development should be devoted to improving low disk loading vehicles which have already proved their worth in a variety of ways and which have tremendous potential for still greater utility.

So much for the things that I'm disturbed about. In a way, I'm like the preacher who berates his congregation each week for the immoral and uncaring behavior of society as a whole. Actually, the preaching is generally more applicable to the sinners who never set foot in church. So perhaps I should conclude with a few things that I'm pleased with.

I'm very pleased to see conferences such as this held which address themselves to much needed research of the kind that I've talked about. You are to be congratulated for tackling difficult and unglamorous problems in which progress is necessarily slow and uncertain.

In going over the program agenda, I am pleased to note that not only are the research topics relevant to what I've discussed earlier, but there appears to be a proper mixture of theory and experiment. With modern computers, we need not be limited to simple momentum considerations and two-dimensional, steady, linear airfoil theory as the basis for aerodynamic analysis. We can now use the more sophisticated tools applicable hitherfore to the more simple fixed-wing aircraft. But please continuously compare theory and carefully thought-out experiments and aim the big guns of sophisticated analyses only at the obvious shortcomings of the simpler theories. In the long run, maximum progress will be made, as it has in the past, through a physical understanding which permits an idealization of the complicated aerodynamic and aeroelastic flow phenomena inherent in rotor operation.

Finally, I think that we can all take pride in the fact that the helicopter and its derivative V/STOL aircraft have served and will continue to serve in a constructive and life-saving role, not only in civil, but in military operations as well. Twenty years from now, I hope that the 1980 decade will be known as the "peaceful eighties." If your grandchildren ask you then as to what you did in the "turbulent sixties" and the "transition seventies," you will be able to point to solid accomplishment in helping to solve some of the problems of our complex society.

SESSION VI

Panel Discussion

Recommendations for Future Aerodynamic Research

Chairman: *John E. Yeates*
Aeromechanics Division
U.S. Army Aviation Materiel Laboratories
Fort Eustis, Virginia

FRIDAY AFTERNOON
20 JUNE 1969

SUBJECT: "NOW IS THE TIME FOR AERODYNAMICS TO COME TO THE AID OF THE HARDWARE" ©

PANEL DISCUSSION

CORNELL/AVLAB SYMPOSIUM

June 1969

Edward S. Carter
Chief of Systems Engineering, Sikorsky Aircraft

The aerodynamics and aeroelastic behavior of lifting rotors must surely claim some records for longevity as an area of continuing and sustained attention in an otherwise fast moving technological society. While flight technology has advanced from Lindbergh to Apollo 10 in just 40 years, the fundamental rotor theory of Glauert has just recently been extended to a free wake analysis (Figure 1). The fact that we can still find surprises in the simple matter of rotor static thrust, that the limits to which rotors can be pushed is still far from clearly defined, and that the vibration and acoustic excitations of rotors remain to be accurately quantified does not seem to speak well for the rotary wing research effort of the past 40 years. And yet, we really have no need to apologize. All things considered, there is probably no other problem in any of the applied sciences short of medicine where so many degrees of freedom participate, where the aerodynamic and elastic elements are so completely interwoven, and where the secondary effects so frequently predominate. In short, we have the nittiest-grittiest problem of them all, and the fact that we are still at it is nothing to be ashamed of.

Now, at last, it does appear that we are really beginning to get our teeth into the problem in all its multi-faceted complexity. The technical literature of the last two years reveals far more tangible evidence of real progress than

in the previous thirty years. Not since the early momentum and blade element theory formulation have any real advances been made in our basic understanding of the rotor performance problem until the last four or five years. Some of this lack of progress was undoubtedly due to a general feeling that we knew all we needed to know to get along. It was only with the advent of high disk loadings, blade loadings, and number of blades - make necessary by demands for greater payload, speed and riding comfort - that the validity of some of the earlier simplifying assumptions was destroyed. Although our complacency can account for some delay, the major factor pacing our technology has been the availability of high-speed computers with the capability not only to model the problem in the necessary detail, but also to process efficiently the vast amount of data necessary to digest our test programs and correlate the analytical results.

Now, where do we stand today? For purposes of discussion, Figure 2 conveniently divides our problem into three basic elements:

First, there is the air mass mechanics. How does the air mass, responding to the forces and torques of the lifting surface, react to generate the inflow environment the lifting surface must operate in? And how is this generated inflow affected by the proximity of other bodies (or rotors)? And finally, when we understand the steady state, how does this complex interrelationship react to dynamic change? These questions have proved to be far more complex than originally anticipated. The comforting concept engendered by the momentum theory of large columns of air advancing serenely through the hovering disk, becoming complicated only when descent velocities approached those of the induced flow or where transition was made from or to forward flight, was destroyed for all time the first day a vortex was discovered tangling with the following blade in a hovering rotor on a windless day. The blade-vortex interaction problem is particularly severe when blade

loadings and number of blades are high. Such a case is illustrated in Figure 3, which shows a vortex vapor trail interacting with a following blade on a heavily loaded, six-bladed rotor system. The technical reaction to this discovery has been excellent, as the multiplicity of papers on inflow and wake analysis attest. It appears that we are making great strides toward modeling the problem analytically and measuring the phenomena in our test facilities. Of course, we have just begun; the work is so expensive that it will be some time before we obtain a complete grasp of even the static unobstructed case, and then we must tackle the effects of airframe and ground plane interferences and the non steady state conditions.

The second area of our problem is the purely aerodynamic response of the local lifting surface element to the air mass. Assuming we can fully define the magnitude and direction (radially as well as tangentially) of the local free stream, precisely what forces and moments will be generated? Here, the problem has been that, unlike for the fixed-wing aircraft, we have a truly bivariant environment; almost any combination of angle of attack and subsonic Mach number can occur. The new information concerning inflow mechanics now reveals that occurrences of high Mach/high angle of attack situations are not limited to just the high speed forward flight condition. Furthermore, even in a hover unsteady aerodynamics can play a part. Here, too, the data is beginning to come in; airfoil testing is recommencing and much of the old data is being re-examined. But we will need much more: more complete Mach/angle of attack coverage - especially for unsteady conditions; more correlation with measured flight test response; and most important, data on a broader spectrum of airfoils designed to live in the demanding rotor blade tip environment with high drag divergence Mach number capabilities and low pitching moments.

The third area concerns the aeroelastic response of the blade to the forces on it. Although generally recognized as the province of the dynamicist, the aerodynamicist can never ignore it because it plays such a large part in the performance the rotor achieves and the measurements the aerodynamicist makes when he tries to dissect the problem. This problem is, perhaps, beyond the scope of this panel. Suffice to say, the dynamicist has made great strides in modeling the manner in which this vibrating piece of spaghetti we call a rotor blade will respond, but the marriage of these dynamic models with air mass mechanics and airfoil response remains a challenge which will draw on the full capabilities of the best aerodynamicists and dynamicists we can attract to help us.

This brings us to the question of where specifically must we go from here. The previous discussion reveals that there is more to do in each of the basic areas where progress is already being made. But the real challenge will be in pulling together all the elements of the problem to provide us with answers to our specific design problems. This is where the surface is barely scratched, and this is where the effort must go if our basic work is to be useful.

Let's enumerate some of the practical questions we are still, apparently, a long way from fully answering. My hit parade, summarized on Figure 4, reads as follows:

1. <u>What combination of aerodynamic, aeroelastic, and geometric factors eventually limit the blade loading capacity and achievable figure of merit of the hovering rotor?</u> Compound and convertible aircraft that no longer demand large blade area for forward flight will place a greater premium on high

loadings. How far can we go? Must we accept a significant degradation of hover performance in order to minimize blade area to save weight and drag at high speed? What special demands does operation at high blade lift coefficients place on the rotor's aeroelastic characteristics to avoid divergences or instabilities? How do we explain the so-called "single rotor blade slap" in the symmetrical hovering rotor case? How do we account for dual tip path plane occurrences noted as some rotors are pushed beyond normal operating limits? These are the questions our models must be geared to answer and only an integrated approach to air mass mechanics, airfoil characteristics, and blade elastic response can hope to do the job.

What part does the number of blades play in the problems cited above? As helicopters become larger, and shaft rpms go down to hold tip speeds to reasonable values, a greater number of blades become desirable to keep the blad passage frequencies above the range where body resonances become a problem. Basic theory tells us we should be able to improve aerodynamic performance with more blades, but experience has not always supported theory. What must we do to gain this figure of merit advantage as we add more blades?

2. <u>The high speed limit capabilities of our rotors and the design factors in-influencing them need much better definition.</u> There is considerable hope that we can push to higher advancing tip Mach numbers and advance ratios than originally anticipated, and the control load build-up appears to be succumbing to stall flutter analysis. In our multi-bladed rotors, particularly, there now appears to be a significant range of airspeed and load factor above the onset of theoretical blade stall which is quite usable, especially for dash requirements. But in this region our power required and trim predictions get into severe difficulties. Also, several rotor misbehaviors have been reported in

this region which may or may not have been satisfactorily explained. If we are to get all the performance we can while retaining an adequate safety margin from potential rotor stability problems, we must keep this area on the R&D agendas for some time to come.

3. <u>What is the true nature of the vertical drag/thrust recovery phenomenon and how do we best capitalize on it?</u> Model tests show that a rotor operating over a body recovers a significant portion of the down load on the body in greater static thrust for the same power. Figure 5 schematically illustrates this thrust recovery phenomenon. Full-scale test instrumentation, however, is generally inadequate to distinguish between vertical drag and thrust recovery, and since we do not fully understand the mechanism of this recovery, we cannot correctly predict the way it will be affected by the relative positioning of rotor and body, the shape of the body, or the degree to which the rotor is approaching drag divergence. This phenomenon is especially important to the hover efficiency of compound and composite aircraft with large wing and empennage areas.

4. <u>For the high speed problem, as well as the hovering vortex proximity case, we need to fully explore the potential of transonic airfoils.</u> Our basic problem is finding the means of postponing compressibility drag divergence with an airfoil that has tolerable pitching moments and also satisfies our retreating blade C_{Lmax} requirements. However, in our efforts to develop new airfoils, we should not overlook the fact that there is very little reliable high Mach number data for existing airfoils. Figure 6 indicates that unsteady 0012 data is required beyond Mach .6, and steady state 0012 data is required beyond Mach .75.

How does a vortex close to or impacting on an airfoil affect the local forces

and moments the airfoil produces and can we find airfoils that excel in forgiving the presence of vorticity? We must start with better definition of the velocity distribution and core dimension of the vortex and the factors which determine them, and we must determine what the "forgiveness" of different airfoils is to the high velocity gradients produced. As an aside, we need to understand the part this interface problem plays in the impulse noise phenomenon.

5. <u>The stability and control of our high speed designs, and the problems associated with rotor/airframe interference are still in need of design resolution.</u>

The stability and control of our high speed designs is still too much of a hit-or-miss proposition. We are beginning to suspect that high speed rotor dynamic stability derivatives do not correlate as well as they should with theory. As a start, we need some good dynamic derivative test data, (presumably separating rotor and airframe contribution), and when we have it we may also find that this is another area demanding more advanced rotor analytical treatment.

In addition to the vertical drag questions, there are the forward flight problems of rotor/airframe interference needing design resolution. This area is particularly important when the rotor is unloaded by large wings and/or auxiliary propulsion. Here is a real challenge to the total system applications of aerodynamics tools. Several anomalies are suspected, particularly in such characteristics as rotor/pylon drag and the dihedral and weathercock characteristics of combinations of rotors and airframes. Figure 7 illustrates the fact that our analytic tools for predicting the impact of rotor/airframe interference on dihedral characteristics are inadequate. Again, we must start with more accurate definition of the problem from wind tunnel or flight tests which can separate rotor and airframe contributions.

6. <u>The attention to meticulous instrumentation to study our problems has been very encouraging in recent years, but we need much better instrumentation if we are to optimize our hardware design.</u> The ability to measure the local angle of attack (or at least the stagnation point as an indication thereof) should be a routine part of our rotor development test programs, at least until wake structures are more rigorously defined. The response of rotors in turbulence also demands some very careful instrumentation to determine what turbulence outside the influence of the rotor is being encountered and then how this turbulence is filtered before it impacts the local section. Another straightforward, yet elusive, instrumentation requirement is the accurate measurement of rotor shaft thrust. We must be able to separate, in flight programs, the airframe and rotor forces and moments. Finally, much more elaborate acoustic instrumentation is needed to separate the contribution of individual noise sources in a rotor and to make acoustic observations the compliment to other aerodynamic measurements that they ought to be. For instance, why can't we develop a microphone array on the rotor head that can focus on the vortex noise from a single blade and correlate it with the other instantaneous measurements such as angle of attack or tip vortex location as revealed by contrails?

7. Finally, in quite a different vein, there are two overall system questions of importance to the design aerodynamicist which deserve mention. They are:

a) <u>What are the local gusts, downdrafts, side winds, etc., that we must design for if we are to operate in the proximity of large buildings?</u>

Where is a program to define this environment and at the same time to suggest reasonable limiting constraints on turbulence allowable for any candidate heliport site?

b) <u>What power margin is really required for operations from a confined area, and how is it influenced by size and configuration?</u> Perhaps we need 500 fpm rate of climb on top of an OGE capability in a 40 ft. diameter craft operating between 50 ft. trees. But do we need this in a 50 ton HLH with a 180 ft. diameter rotor which will still be in ground effect well above the trees? Figure 8 suggests this size trend for single rotor helicopters. The plot shows the percentage of OGE hover power required for takeoff over a 50 ft obstacle in 200 ft for several aircraft as given by their flight handbooks. More data is needed to pin down the actual power margins required for safe operations to confirm this size effect and also to determine the impact of rotor configuration on hover margin requirements. Undoubtedly we also have some basic things to learn about ground effect when we are dealing with a ground plane obscured or interrupted by large masses of vegetation.

The list, of course, can go on almost without end, and there are many other areas where current research needs continued attention. I have no intent to try to cover them all. The principal point that should be stressed is that we are making good, if belated, progress in understanding the basic aerodynamics of rotary-wing aircraft. The major challenge is now to apply this basic knowledge to the <u>total system</u> problems we must solve - the limits and efficiencies that basically determine our cost-effectiveness. In the long run our aerodynamic research is only of value to the extent that we translate it into hardware design. Our end item output must, after all, be rotor geometry and body geometry recommendations, and the tools to predict the results of our selection. The time has come to really bear down on our hardware decisions with our developing spectrum of new tools.

Figure 1. MILESTONES

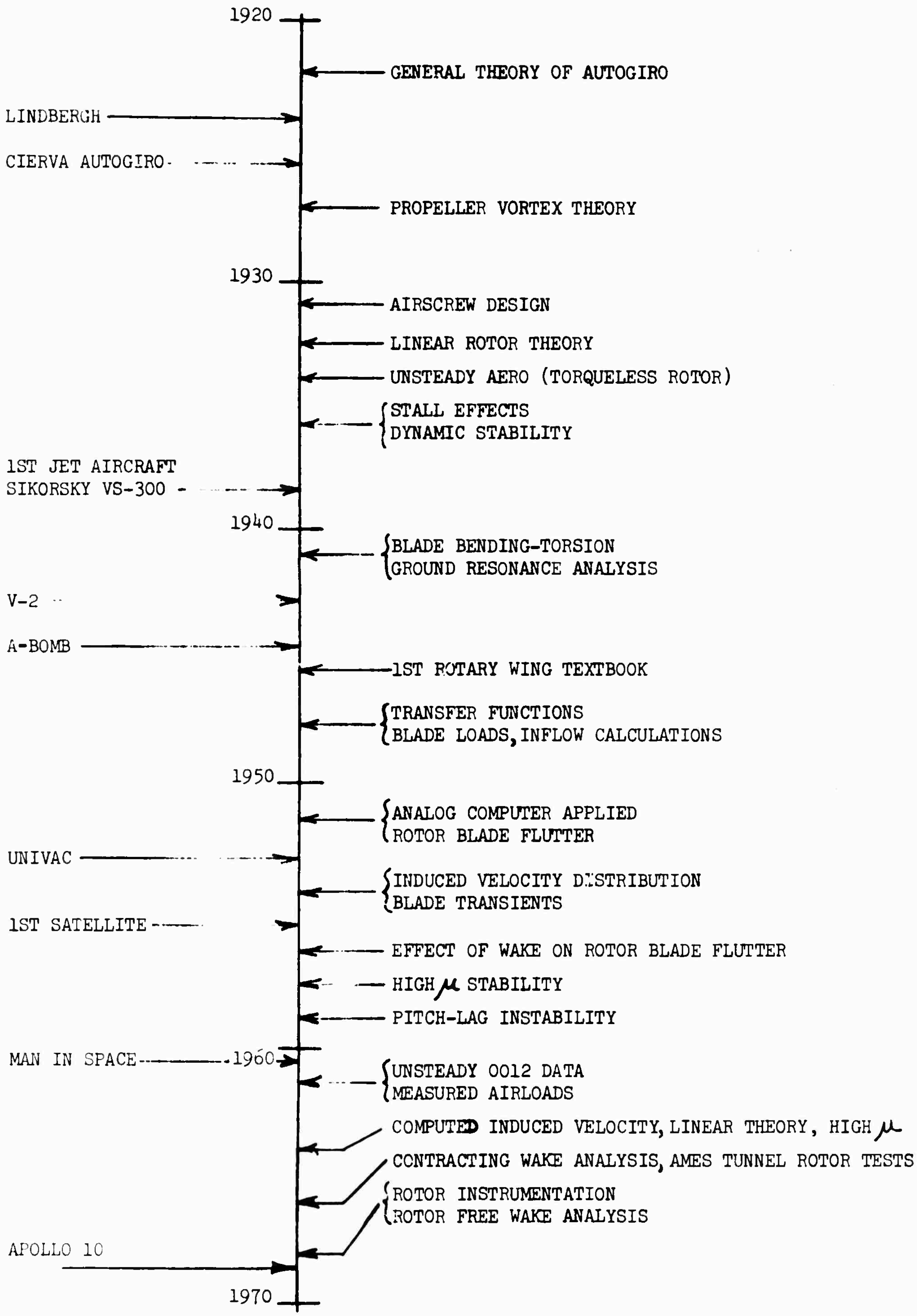
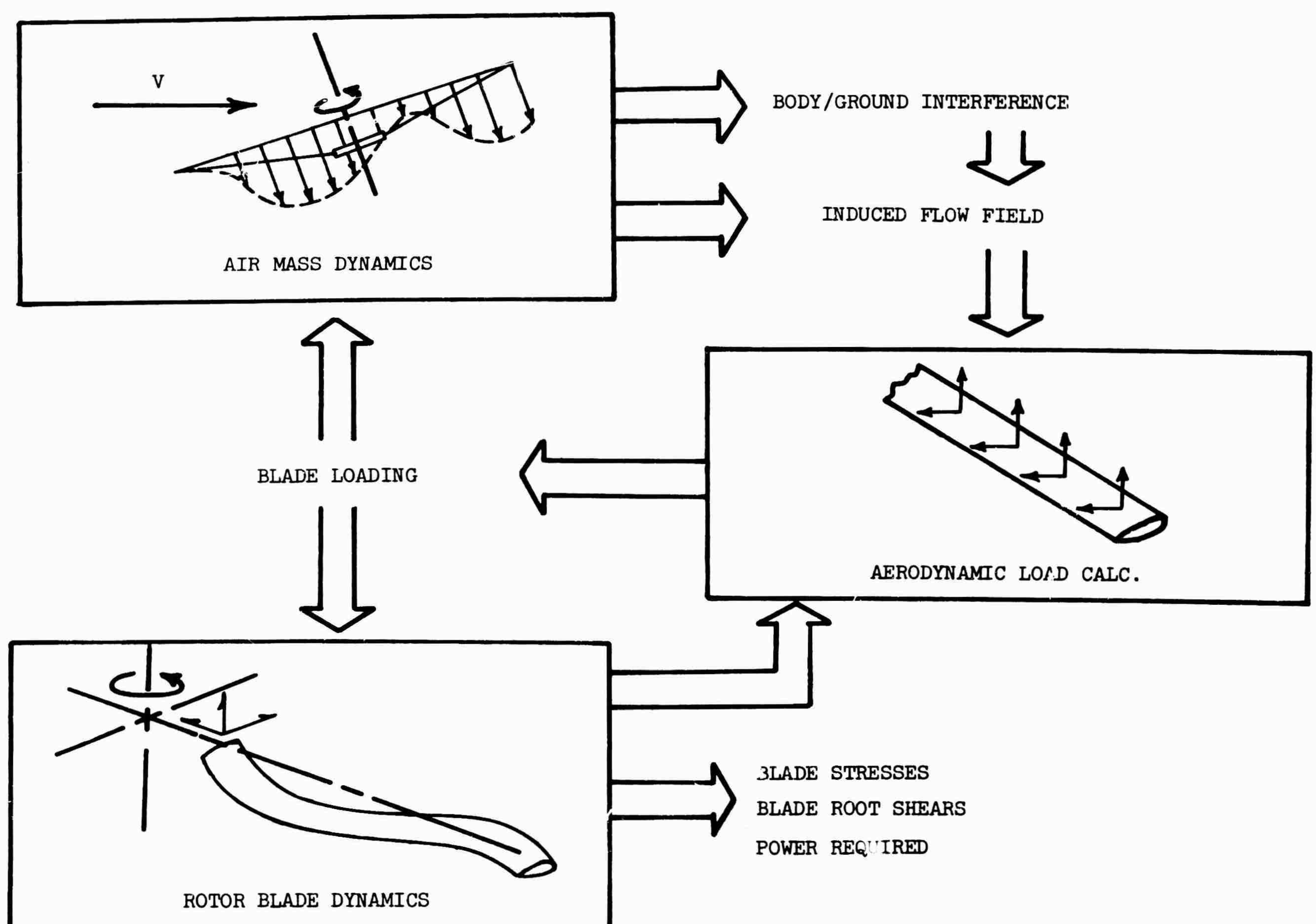


Figure 2. BASIC ELEMENTS OF THE PROBLEM



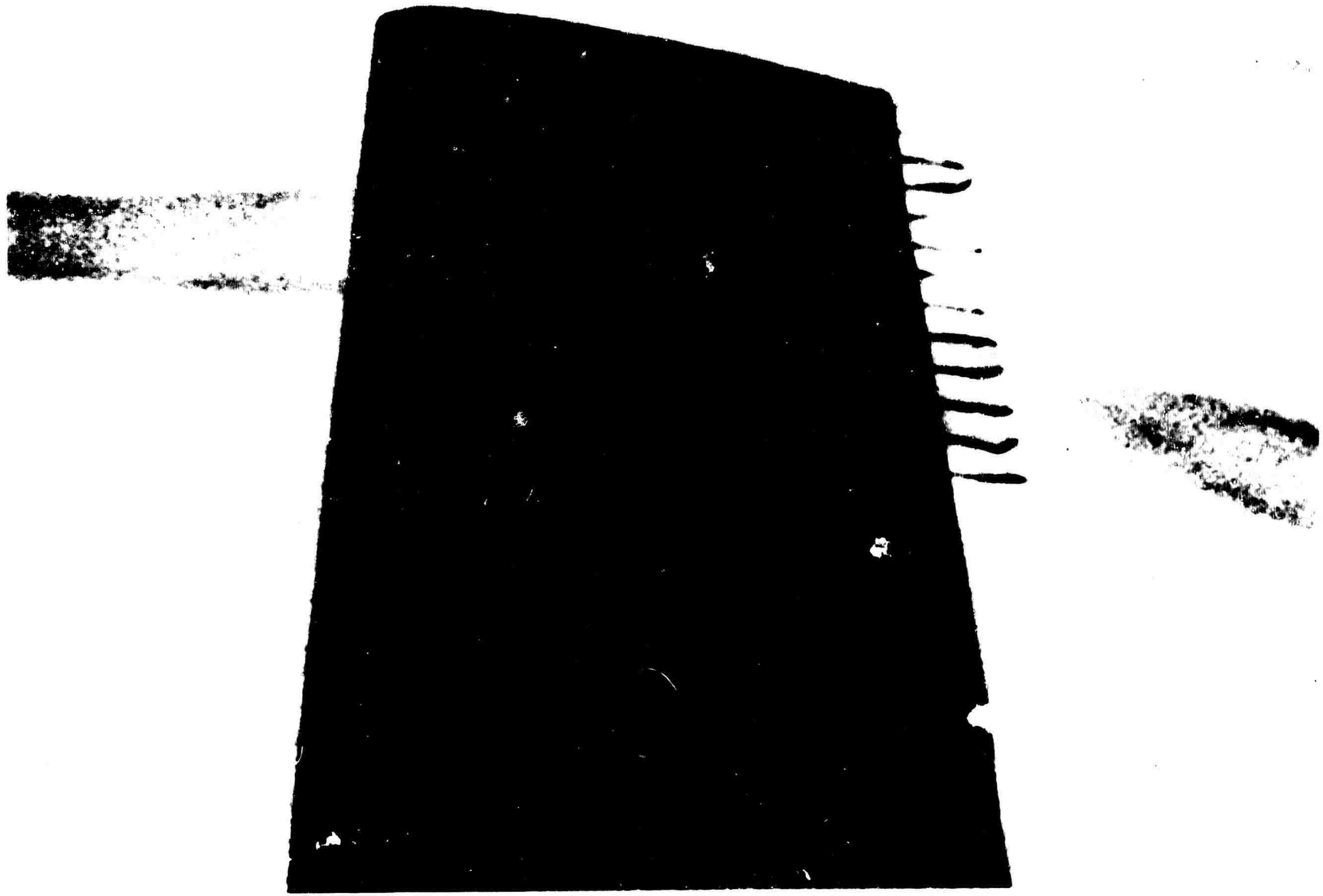


Figure 3. TIP VORTEX CONTRAIL PASSING OVER FOLLOWING BLADE - SIX BLADED ROTOR ON SIKORSKY TEST STAND

Figure 4. UNFINISHED BUSINESS

1. HOW FAR CAN WE PUSH C_T/σ IN HOVER?

   - Mach? Twist? N?

2. HOW FAR CAN WE PUSH INTO STALL IN FORWARD FLIGHT?

   - Control Loads, Stress and Vibration, Power?

3. HOW DO WE MINIMIZE VERTICAL DRAG?

   - Thrust Recovery Phenomena

4. HOW DO WE OPTIMIZE OUR AIRFOILS?

   - Supercritical Technology? Vortex Forgiveness?

     $C_{L_{MAX}}$ vs C_M ?

5. DYNAMIC STABILITY STILL PRODUCES UNEXPLAINED ANOMALIES

   - Rate Derivatives, - Interference Effects?

6. WE MUST DEVELOP INSTRUMENTATION ADEQUATE TO CORRELATE OUR ANALYSES

   - Rotor Angle of Attack, Rotor/Body Forces

7. THE VERY LOW SPEED, VERY LOW ALTITUDE OPERATIONAL INTERFACE NEEDS MORE ATTENTION:

   - Turbulence Environment, Power Margin Requirements, Mechanics of Downwash Effects

Figure 5. VERTICAL DRAG AND THRUST RECOVERY

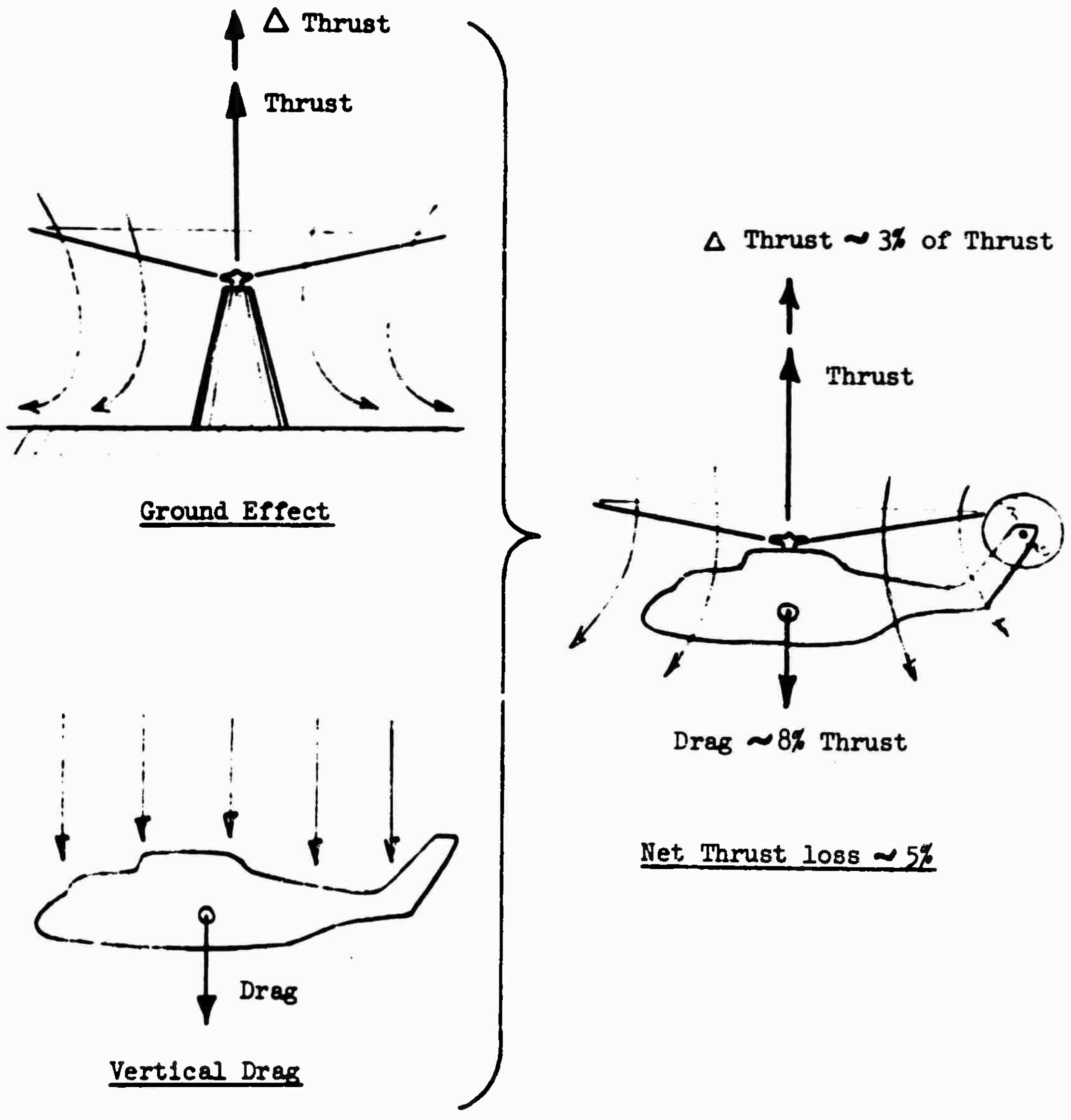


Figure 6. BLADE TIP ENVIRONMENT AND AVAILABLE AIRFOIL DATA

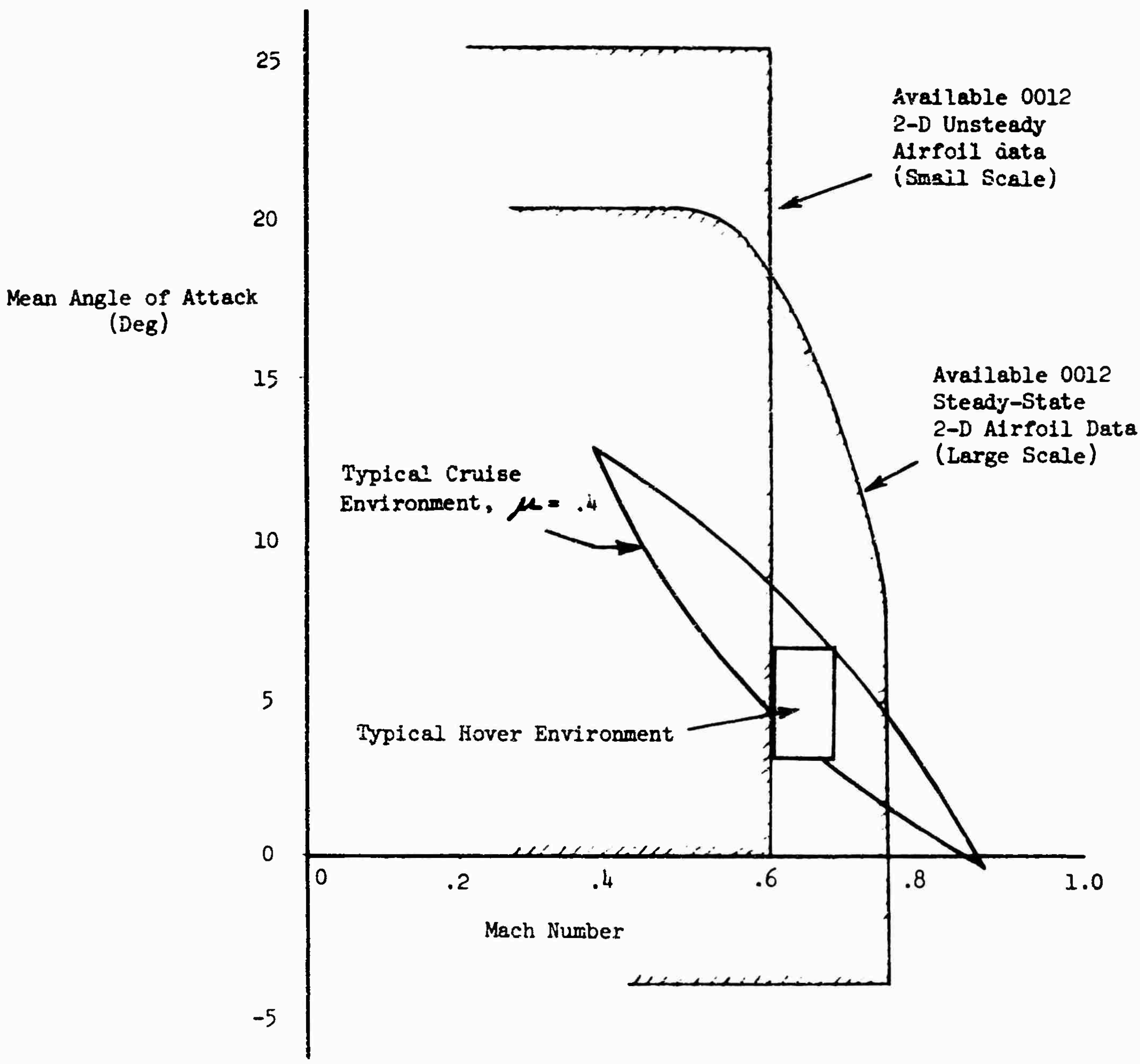


Figure 7. EFFECT OF MUTUAL ROTOR FUSELAGE INTERFERENCE ON DIHEDRAL EFFECT-COMPOUND MODEL - 150 KTS

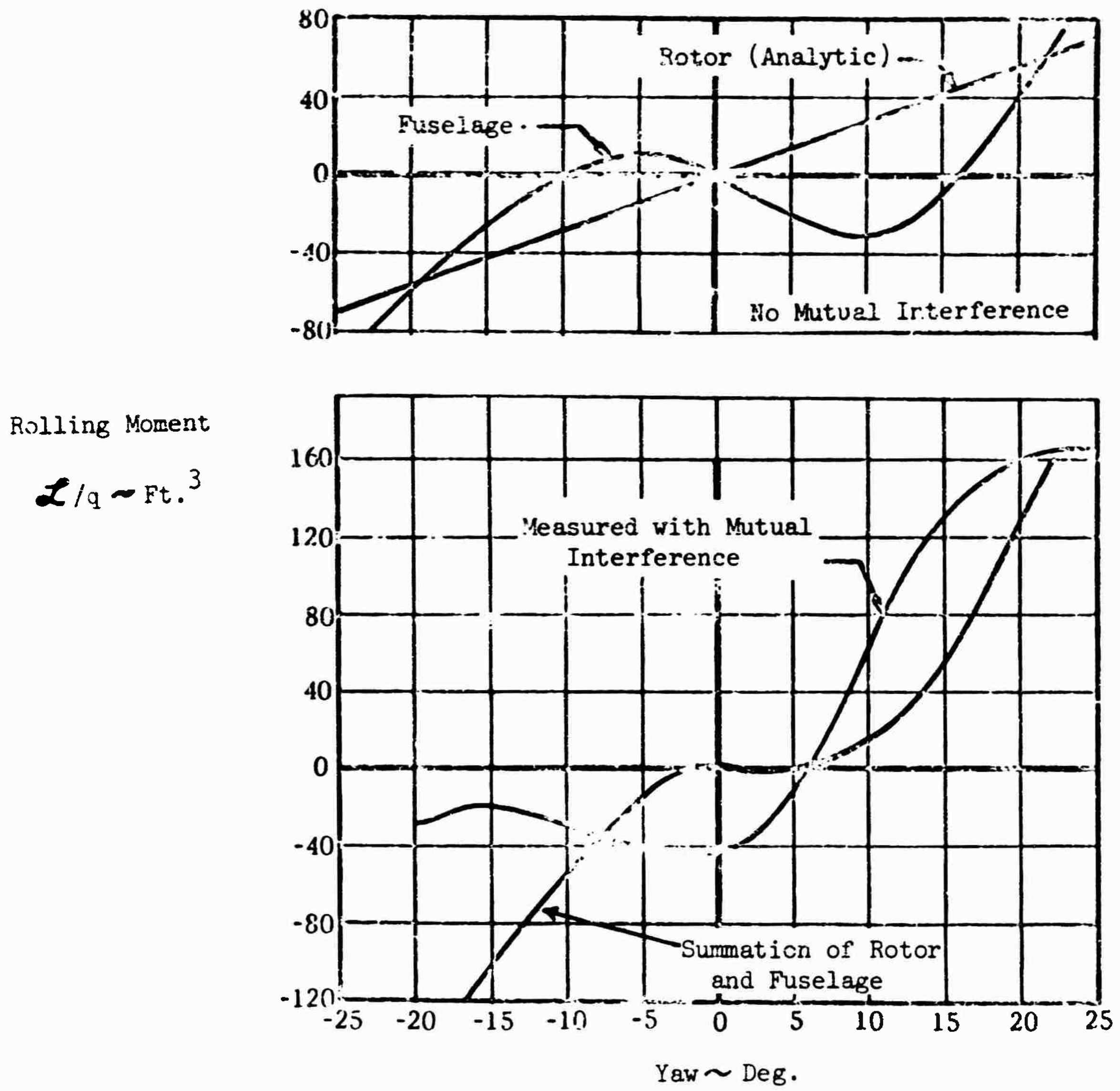
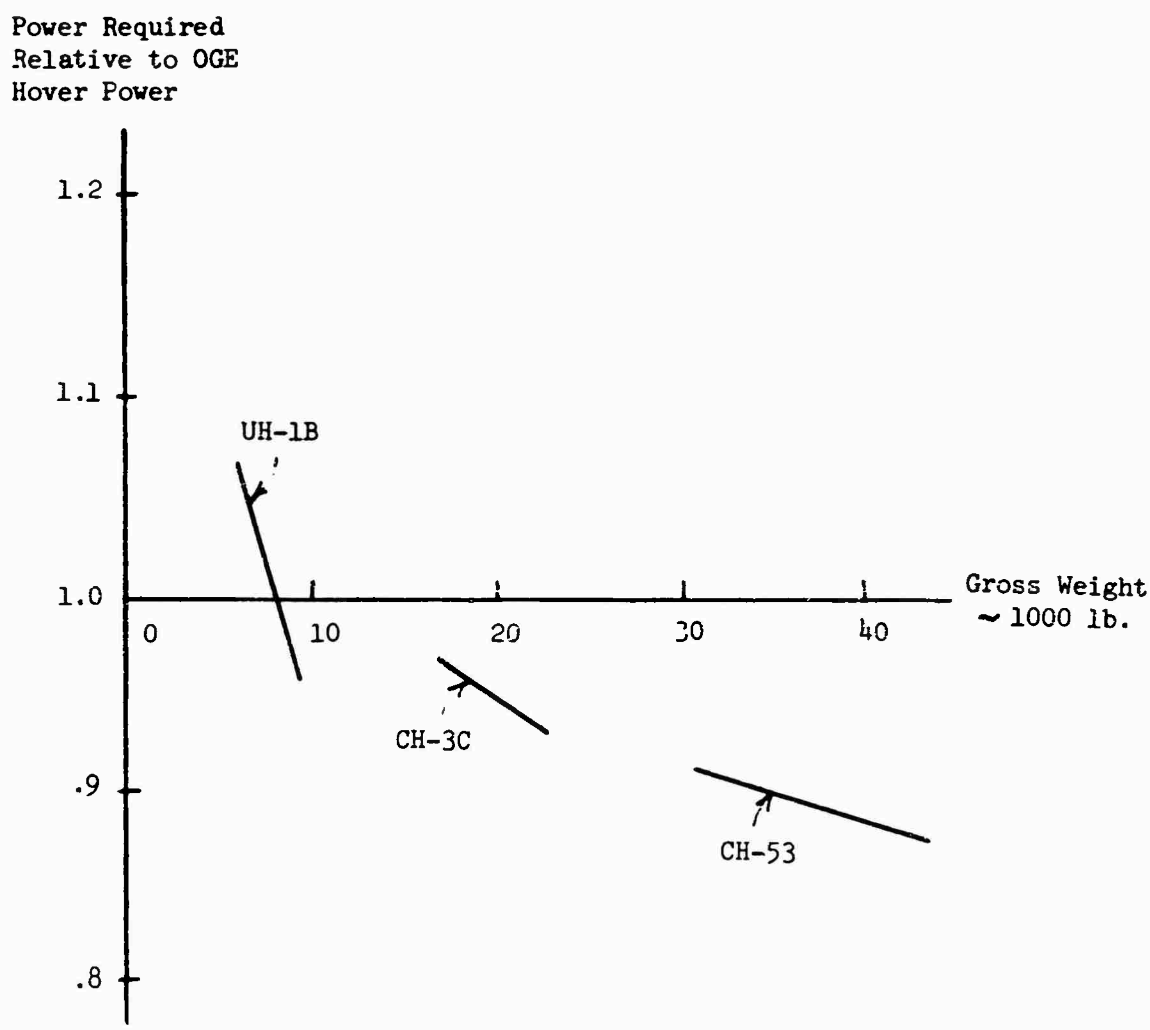


Figure 8. HOVER POWER RATIO FOR TAKEOFF OVER 50 FT OBSTACLE IN 200 FT.

SINGLE ROTOR HELICOPTERS



RECOMMENDATIONS FOR FUTURE AERODYNAMIC RESEARCH

by

C. W. ELLIS

I. INTRODUCTION

The following discussion attempts to outline those areas of rotary wing aerodynamics which currently pose the most perplexing problems to the designer and the operator of such aircraft and those areas where aerodynamic improvements are likely to have the largest payoff in either the development of a new vehicle or in the performance of such a vehicle once it has been developed. Limited data is shown to demonstrate the magnitude and nature of the problem (when that data is available). No attempt has been made to correlate this statement of problems with any list of research currently under way, although it is recognized that in many cases such activities are in fact proceeding.

I. INTRODUCTION (Cont'd.)

The problems are discussed under six general topics:

A. Rotor Systems

B. Noise

C. Downwash

D. Rotary Wing Wake

E. Flying Qualities

F. Wind Tunnel Test Techniques

II. ROTOR SYSTEMS

A. Performance Prediction

The problems in rotary wing hover performance prediction have been identified and discussed at length. This problem has been attributed to lack of detail knowledge about the rotor wake geometry. In addition to this well known area, a serious problem exists in the high speed flight area. Figure (1) illustrates this problem. Performance predicted using the best available performance methodology (which includes consideration of the non-uniform wake, rotor blade flapping motions and two-dimensional airfoil characteristics which vary with C_L and Mach number) is compared with performance measured

A. Performance Prediction (Cont'd.)

in the wind tunnel. As can be seen, good agreement is achieved up to about $\mu = 0.3$. Beyond this, the measured and calculated performance diverge rapidly particularly at high Mach numbers. Attempts to understand these discrepancies suggest that they are associated with the torsional deflections of the rotor system and the high Mach number drag characteristics of the airfoil sections. Some circumstantial evidence also exists to suggest that yawed flow effects may be significantly influencing the results at high Mach number and high advance ratio conditions. Understanding this problem is essential to the development of methods for predicting it and most importantly to development of methods to control it such that the potential performance can in fact be achieved.

B. Rotor Operating Limits

Today's rotor systems are all currently limited in their operating weight/speed/altitude envelope by some manifestation of aerodynamic stall effects. These appear as either a drag increase, a reduction in lift curve slope, or most commonly, major increase in control system loads.

B. Rotor Operating Limits (Cont'd.)

Recent testing of a Boeing-Vertol Advanced Geometry Fiberglass Composite rotor blade has provided a preliminary confirmation of the important inter-relationship between blade structural characteristics and the rate of growth of stall induced blade control loads. Figure (2) compares non-dimensionalized pitch link load versus advance ratio for the standard CH-47C metal spar blade and for the composite blade. Control loads start to diverge from their normal μ^2 relationship at about the same advance ratio, indicating that the onset of stall was occurring at the same speed. However, the rate of growth of these loads with speed increases beyond onset is substantially less for the composite blade. Efforts are currently underway to define the significant structural parameters that create this difference in growth rates. It is suspected that the torsional structural damping characteristics are the significant factor here. Confirmation of this characteristic can possibly lead to a capability for a significant increase in the operational structural envelope for rotary wing aircraft, if we can learnto control the significant control parameters.

C. Rotor Loads

Implicit in the discussions above is the suggestion that we can only properly determine the vibratory loads and forces generated by our rotor systems if we understand our airfoil characteristics in the rotor operating environment, if we properly understand and represent the significant rotor structural parameters, and if we know the description of the air flow passing through the rotor system. I'm sure we can all cite many examples which provide rather convincing evidence that indicates we can't do a good job of predicting these rotor loads in those areas which are heavily dependent on accurate representation of the flow field and which are involved in operation in or near the rotor stall limits. Since our critical rotor load conditions almost always are found in such operating regimes we need to do some considerable sharpening of our analytical tools and improvement of our input data to give ourselves the capability of predicting accurately the critical rotor loads.

D. Rotor Airfoil Development

As noted above, one of the critical elements in the rotor system behavior is the characteristic performance of the rotor blade airfoil. Considerable evidence exists that our current two-dimensional static airfoil data is completely inadequate for purposes of predicting rotor system behavior. Further, the substantial gain in rotor system performance capability achieved by the introduction of cambered airfoils provides one positive piece of empirical data to indicate the magnitudes of the performance improvements that may be achieved by developing airfoils specifically tailored to the rotor performance requirements. I believe it is important that such airfoil development be done in an environment which simulates the critical forward flight environment of a rotary wing airfoil. Figure (3), which compares the static and dynamic characteristics of two common airfoils, one symmetrical and the second cambered, shows very clearly that proper lift and moment behavior of the airfoil can only be determined dynamically. For example, examining the static moment and lift data one would conclude that a 19% increase in operating lift coefficient could be achieved and that lift and moment problems occur together. Making the same examination

D. Rotor Airfoil Development (Cont'd.)

dynamically and using the moment break point as a measure of the rotor operating limit, one can conclude that a 38% improvement can be achieved and that moment problems occur prior to lift problems. By recognizing the source of our fundamental rotor limitations and by optimizing airfoils to provide desirable characteristics to these fundamental parameters, it seems reasonable to expect further significant gains in rotor operating capability.

III. NOISE

In the area of rotor noise we have identified several sources of noise. Rotational and vortex noise sources and variations are becoming reasonably well understood. The high speed Mach number critical advancing blade bang and tandem rotor bang due to rotor-to-rotor vortex intersection are also well in hand and means for reducing these noise sources are understood. Recent whirl tower testing has identified another source of rotor noise which occurs in or very near the hovering regime when proper combinations of tip speed and blade span loading are achieved. This phenomenon which we have called single rotor bang is characterized by the same kind of impulsive

loading typical of blade vortex intersection. It is observed at wind speeds as low as 2 knots. The mechanism for its generation is not clearly understood nor are the means available for controlling this noise understood.

Figure (4) describes the limits we have established by full scale testing of a three-bladed CH-47 rotor. The implications of this source of rotor noise become particularly serious when contemplating some of the stowed and stowed/tilt rotor configurations since with these configurations blade number tends to be limited by the physical requirements for folding. Hence, the blade span loading tends to be high. The optimum tip speeds for such a vehicle also tend to be high and these combinations of high tip speed and high span loading appear to force such configurations into an area in which they are susceptible to generating this type of rotor noise.

Additional testing needs to be conducted to define the mechanism by which this noise source is generated and to determine the effects of other variables such as blade number on the noise boundary.

IV. DOWNWASH

The effects of rotary wing aircraft on their external environment are probably influenced more by the downwash field of the aircraft than by any other single factor. Yet in this area we probably have less solid technology upon which to base our design judgments than in any of the other areas of external influence. The tendency has been to consider disc loading as the single important parameter in determining the effects of the rotary wing vehicle on the surrounding environment yet ample evidence exists to say that this is not the case.

Figure (5), for example, is extracted from NACA TM-3900 and shows the predicted wake pressure signature for a two-bladed and a four-bladed rotor having the same thrust and the same disc loading. It is obvious that the pressure peaks and hence the influence of the two-bladed wake is substantially greater than that of the four-bladed wake. Here then is an example of the overriding impact of blade number parameter (or possibly span loading) on the effects generated by the rotor wake on objects in close proximity to the rotor.

Figures (6) and (7) summarize the available data concerning the influence of rotary wing wake on objects which might be described as far field objects, i.e., objects greater than

one rotor diameter away from the center of the rotor. This probably includes the majority of the items which are affected by rotor downwash in the normal operational situation. Here we see that the forces and moments on objects in the wake are only slightly influenced by disc loading; in fact at a constant physical distance, a high disc loading vehicle generates a lower force and moment on an object than a low disc loading vehicle. On the other hand we do see a strong influence of gross weight, independent of disc loading, on the forces and moments generated on an object in the wake. (It is interesting to note that since the relationship between span loading and disc loading increases with the radius and hence with gross weight at constant disc loading, we would see a similar trend of the peak pressures as displayed in Figure (5) with gross weight.)

Our technology does not give us the capability to successfully integrate the effects in order to arrive at a design specification which will insure acceptable downwash environment. We can currently only achieve this through empirical testing. Even there our ability to extrapolate properly to the full scale effects is questionable. If we are to design heavy lift

IV. DOWNWASH (Cont'd.)

vehicles and large transport vehicles which can operate successfully in close proximity to people and buildings, we must make better sense out of the data that exists in this area and develop means for correlating these data with known experimental results.

V. ROTOR WAKE

In all the areas discussed above this single item has occurred repeatedly in discussions of our ability to predict rotary wing behavior. While it is probably obvious to everyone, I think it is worth repeating and emphasizing the importance of a thorough and complete understanding of the rotor wake, both its geometry and its strength characteristics. As shown in Figure (8) the rotor wake has a major influence on almost all aspects of rotary wing aerodynamics. We have gross methods of rotor wake analysis and representation but we are becoming more and more aware that these methods leave some significant opportunities for error in many important aspects of the system. Without dwelling on each of the interactions shown on Figure (8), I think it is sufficient to say again that until we understand the details of the rotor rotary wing wake we will not have a satisfactory capability for understanding and predicting behavior of our rotary wing craft.

VI. FLYING QUALITIES

In the area of rotary wing flying qualities, we have made one significant accomplishment in the past 18 months. This accomplishment, graphically represented in Figure (9), has been the demonstration on two different helicopters of the ability to successfully simulate, on a moving base simulation system, the flying qualities of these aircraft. This simulation was of sufficient fidelity to allow reasonable pilot judgment to be made regarding the suitability of the flying qualities for a variety of flight conditions. This confirmation of the potential validity of moving base simulation appears to open an interesting avenue of speculation concerning means to provide rotary wing aircraft better tailored to the missions for which they are to be used. I have attempted to show in schematic form in Figure (10) how this technique might be used.

It has been generally accepted for some time that the flying qualities requirements should be tailored to the particular prime mission of the vehicle under consideration. For example, it is unlikely that the flying qualities which are optimum for a weapon platform will also be optimum for a transport aircraft or for an ASW type aircraft mission. However, we have been

unable to translate this intuitive knowledge into a cohesive set of handling qualities specifications. Work is underway in that area. Perhaps equally important is the effect of configuration on the stability and control requirements necessary to meet the particular set of mission flying qualities requirements. For example, a vehicle which is not sensitive in control requirements to side winds and tail winds could be expected to have a significantly different control requirement than an aircraft which has a high sensitivity to such wind conditions. Again, our specifications have given us no technically substantiated method for handling this kind of situation.

It would appear possible that the moving base simulation technique offers us the opportunity to combine candidate configurations with specific mission requirements and with past history and specifications to arrive at a set of specific vehicle stability and control requirements which would insure that the handling qualities of the vehicle are satisfactory for meeting the requirements of the aircraft's mission and that the stability and control characteristics meet the flying qualities objectives. Further work in this area may be

VI. FLYING QUALITIES (Cont'd.)

expected to significantly improve our ability to provide vehicles with superior pilot ratings and mission performance.

VII. WIND TUNNEL TEST TECHNIQUES

Perhaps one of the most persuasive arguments regarding the relative status of fixed wing and rotary wing technology can be seen by comparing the history of fixed wing and rotary wing wind tunnel testing. Figure (11) has been prepared from Boeing Company experience in this area. As can be seen, rotary wing aircraft are typically developed with 10% to 15% as many wind tunnel test hours per aircraft model developed as is typical of fixed wing practice. Further, rotary wing wind tunnel testing at this time is rather heavily concentrated in the performance and flying qualities area and very little work is done in the loads and dynamics areas which are occupying a larger and larger percentage of the fixed wing tunnel budget.

We are making strides toward our goal of being able to determine the performance, stability and control, loads, vibratory forces, dynamic behavior, and the noise of our rotary wing aircraft in the wind tunnel before we get into flight status. The benefits

are obvious; fewer surprises during the flight test program, better performance for the specific vehicle concerned and because of the superior quality of the data available for test/analytical correlation, better methods for predicting behavior of future designs. However, we still have a long way to go before we are able to economically and rapidly achieve the objectives outlined above. Areas requiring particular emphasis are the economics/schedule of models which are dynamically similar in sufficient detail to enable the prediction of loads, vibratory forces and dynamics to be made with confidence, techniques for determination of the aircraft stability and control characteristics, techniques for noise measurements, and techniques for determining the loads and vibratory forces generated by the rotor system. Full development and exploitation of the potentials in this area is probably one of the best ways available to us to insure that the achieved performance on any new rotary wing aircraft will be as good or better than that claimed in the proposal document.

CONCLUSIONS

In the items discussed above I have not attempted to outline the details of any specific research requirements but have attempted to point out some of the key problem areas which stand between the designer and the accomplishment of his objectives. I think it is important to emphasize that these problem areas can have a significant effect on the performance of any new vehicle compared to its proposed or potential performance and therefore a solution to these problems can be a significant factor in reducing the well publicized discrepancies between estimates and reality as well as being important in providing the user with a vehicle more closely tailored to his specific requirements.

FIGURE -

PROPULSIVE EFFICIENCY

(B-67 CORRELATION)

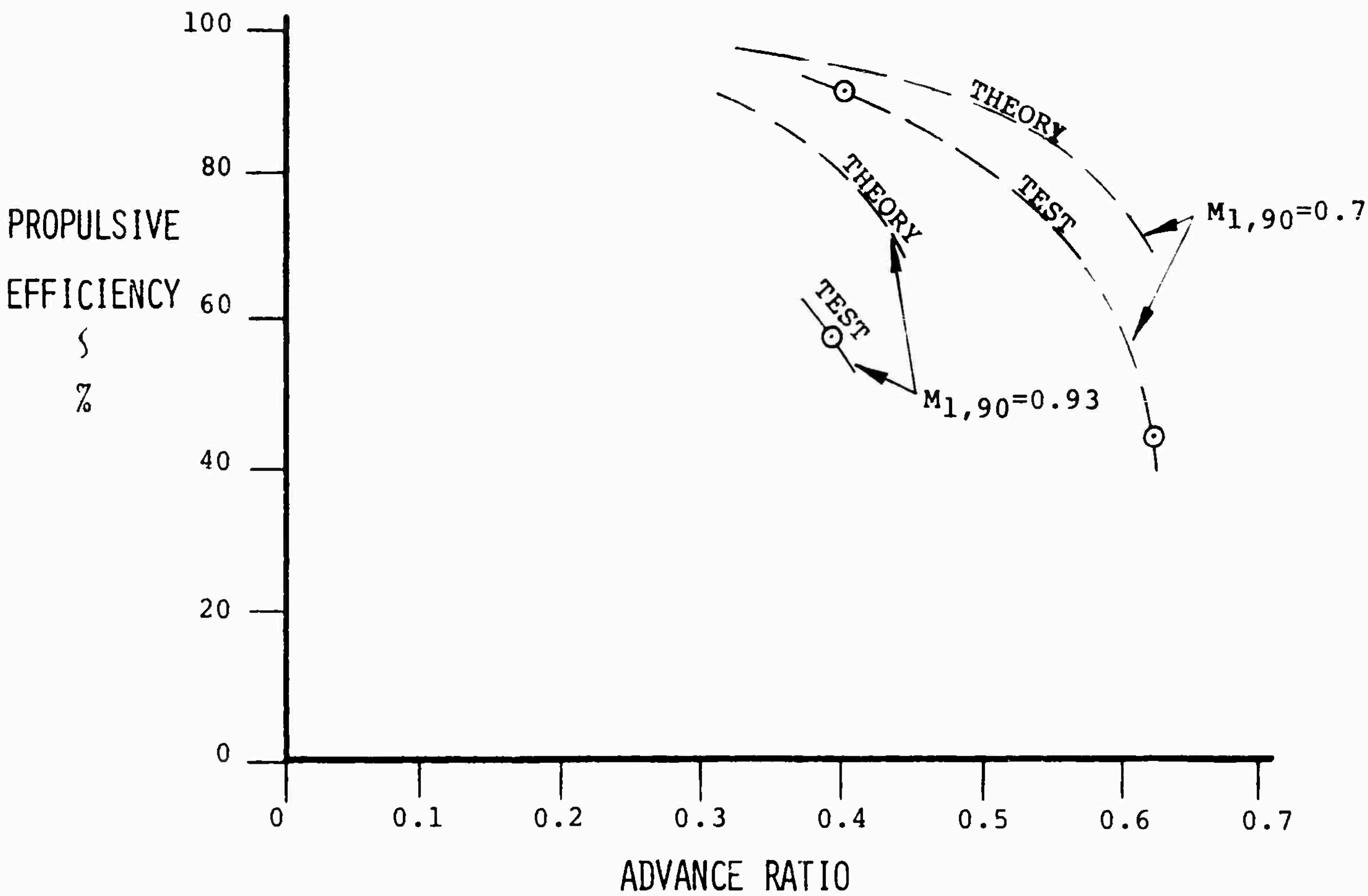
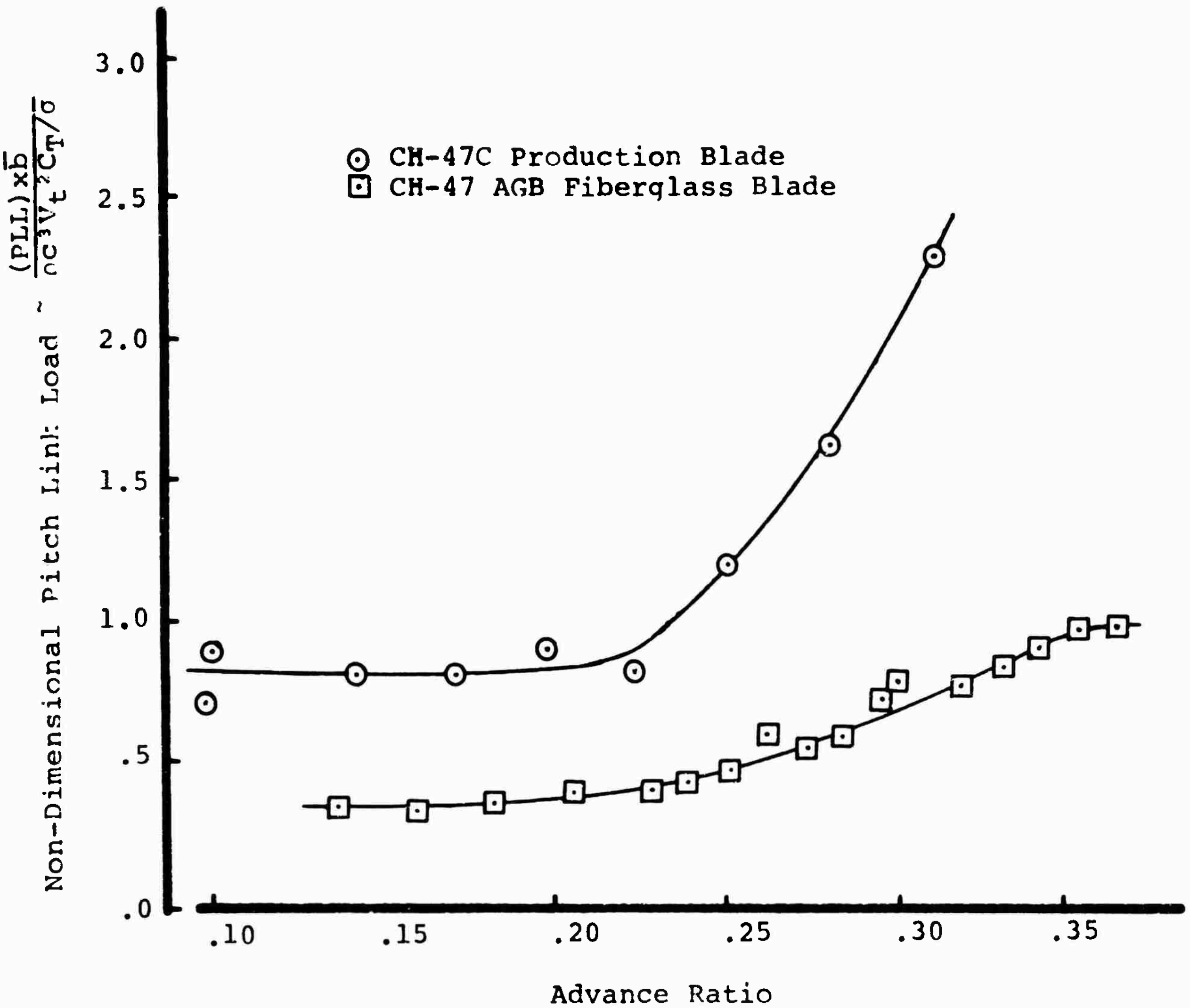


FIGURE 2

ROTOR LIMITS



Aircraft	Flight No.	Gross Weight	RPM	Altitude	CG	Trim
CH-47C	X-65	40,400 Lbs.	235	7,600 Ft.	6"Aft	Prog.
CH-47 AGB	X-255	38,900 Lbs.	235	8,000 Ft.	6"Aft	Prog.

FIGURE 3

STATIC AND DYNAMIC AIRFOIL BEHAVIOR

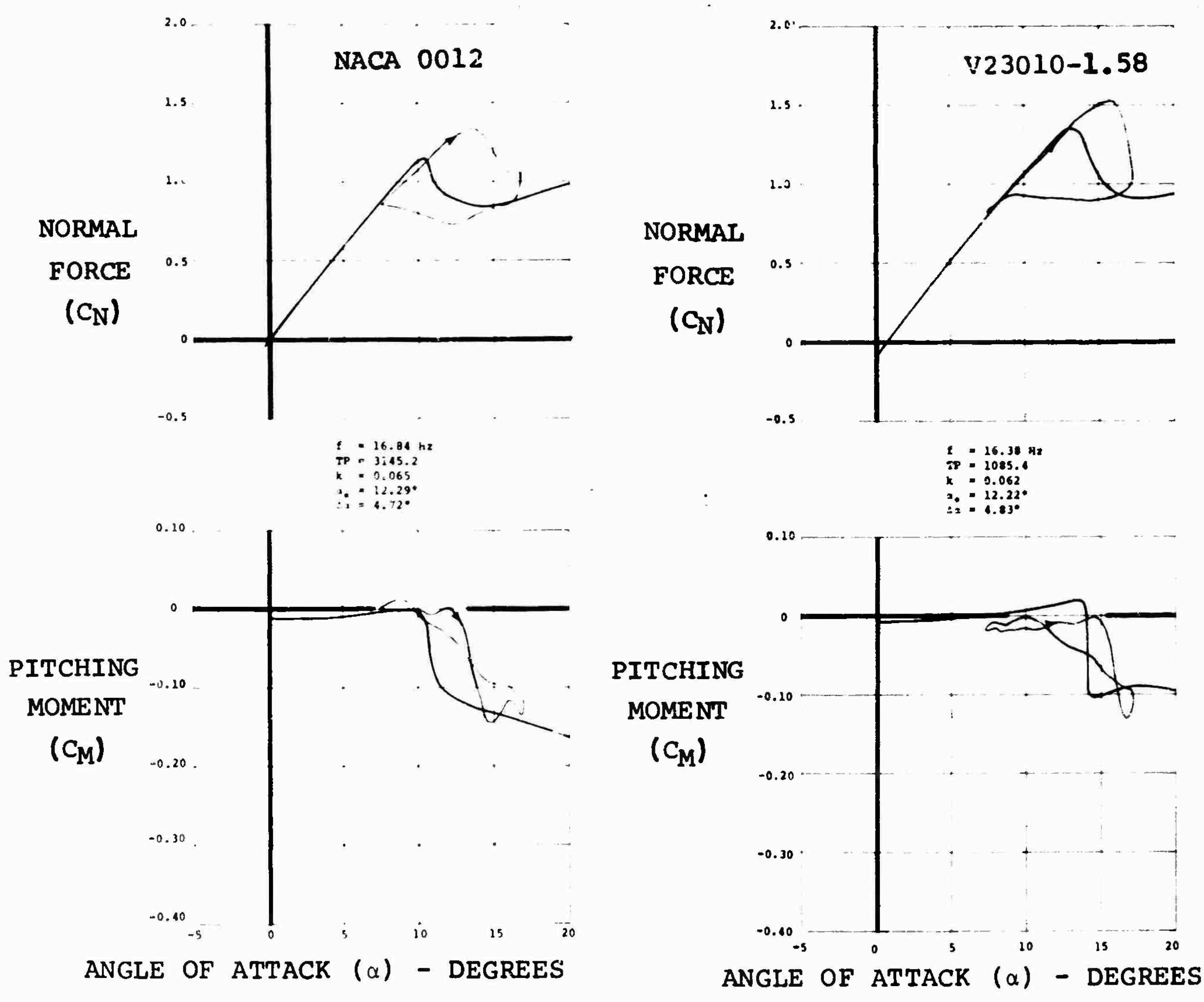


FIGURE 4

SINGLE ROTOR BANG CRITERIA FOR MILITARY HELICOPTERS

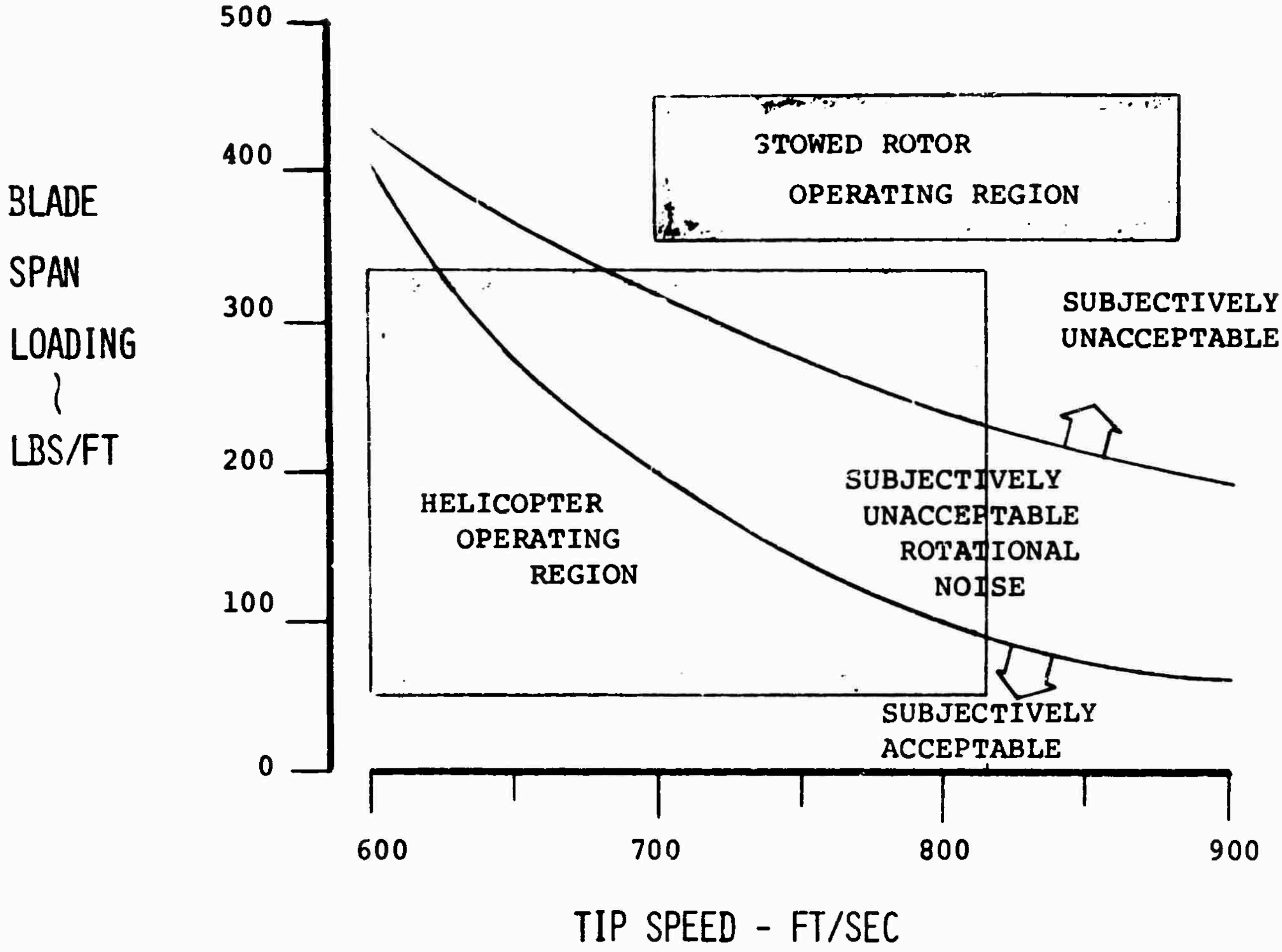


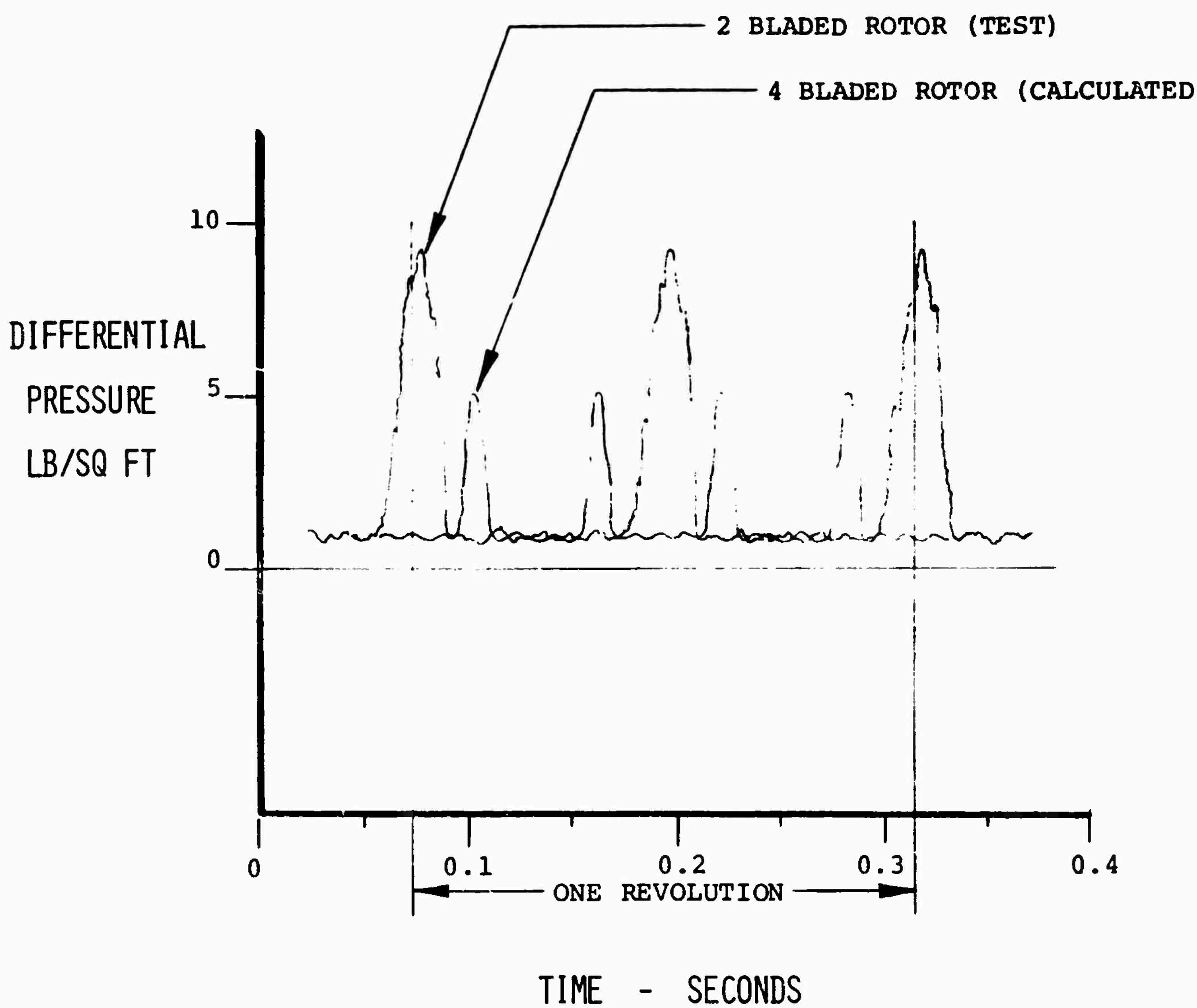
FIGURE 5

DOWNWASH

NEAR FIELD WAKE

DATA SOURCE: NACA TN 3900
THRUST = 2600 lb
DISC LOADING = 2.36 lb/sq ft
$C_T/\sigma = .149$, $C_T = .004$
TIP SPEED = 500 ft/sec

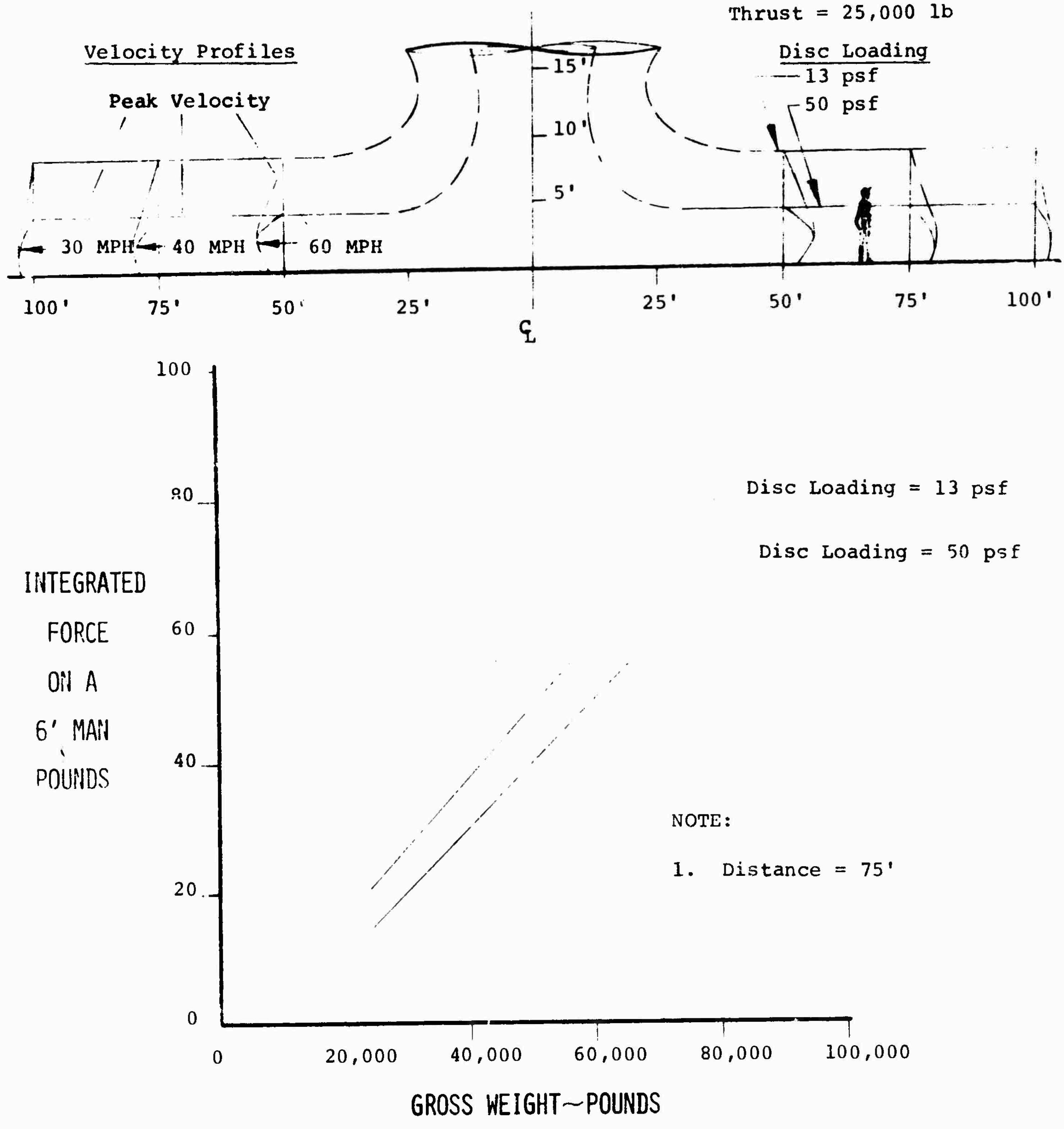
$\frac{Z}{R} = .05$



DOWNWASH

FAR FIELD WAKE

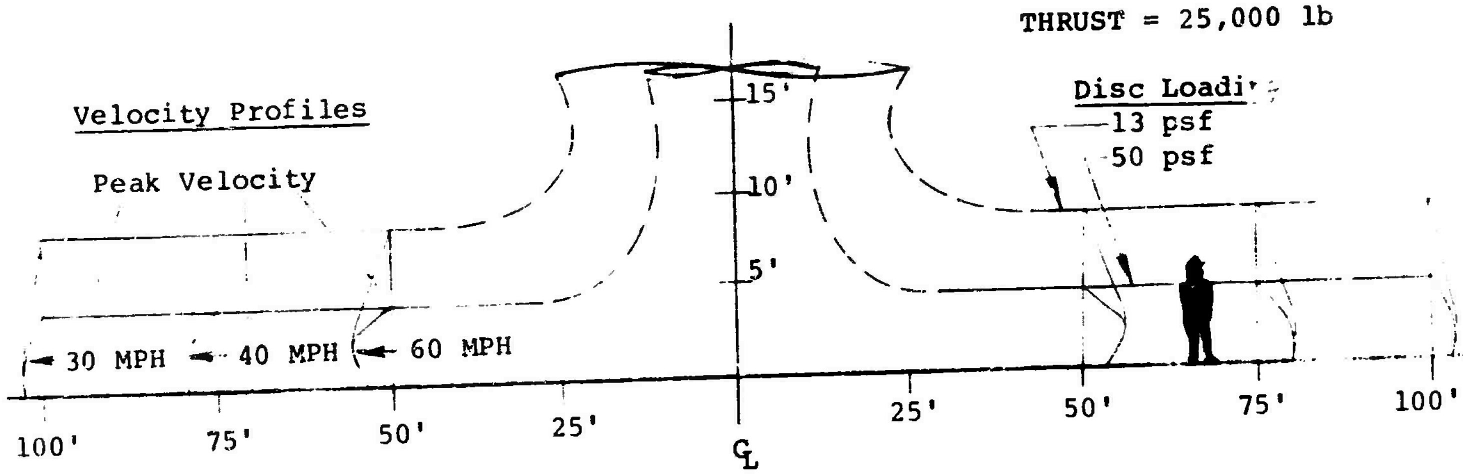
FIGURE 6



DOWNWASH

FAR FIELD WAKE

FIGURE 7



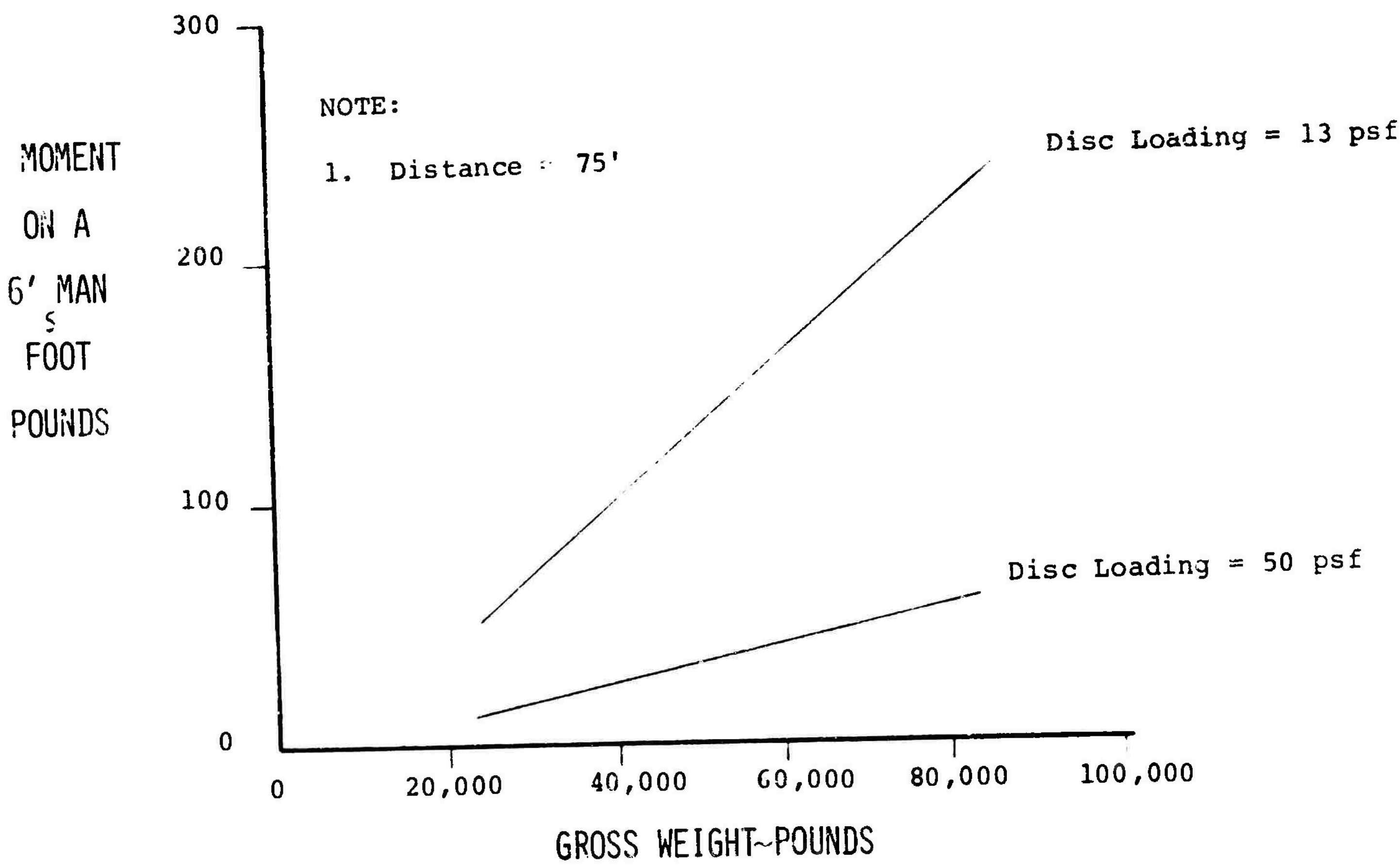


FIGURE 8

AERODYNAMIC INTERACTIONS

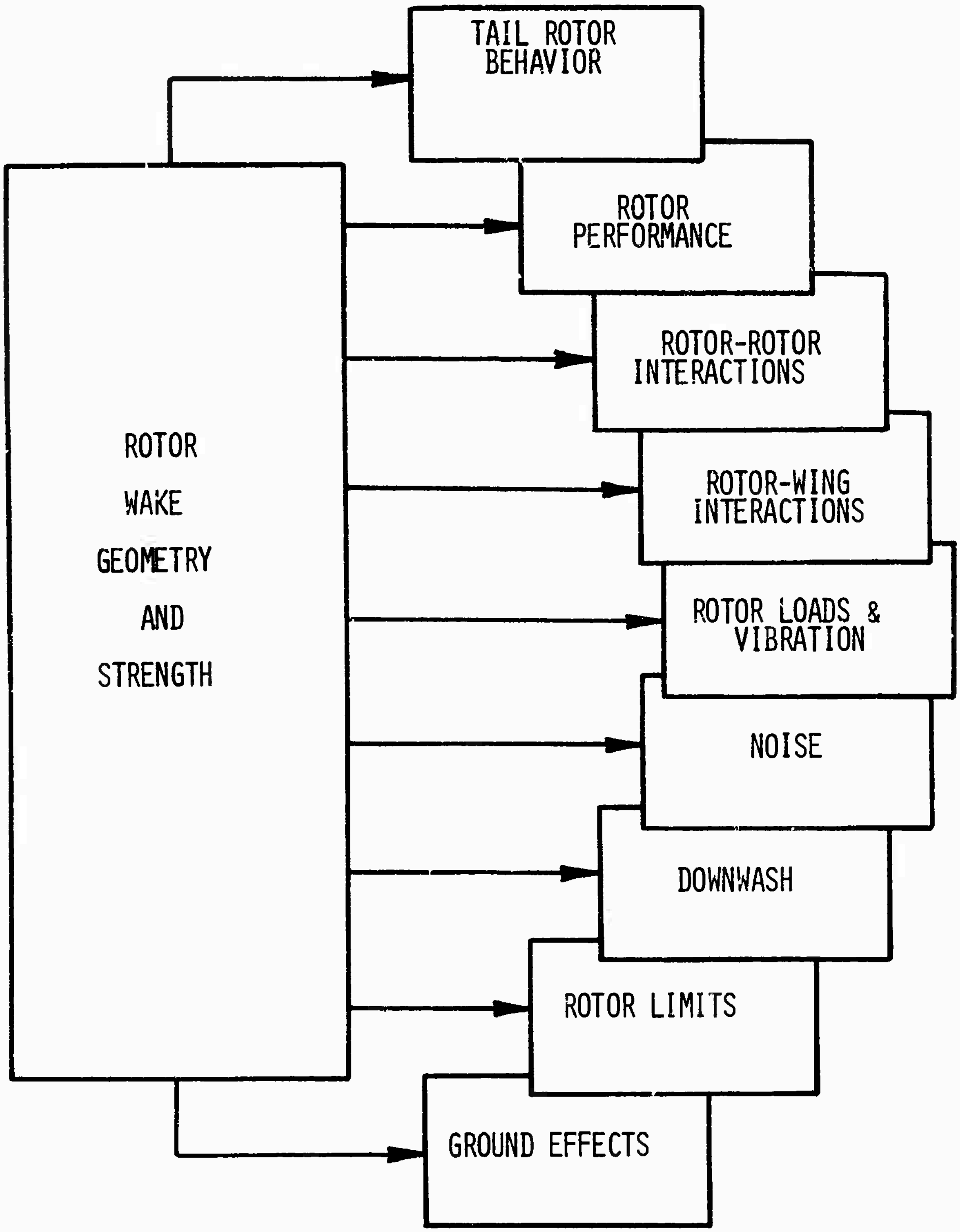


FIGURE 9

FLYING QUALITIES

MOVING BASE SIMULATION

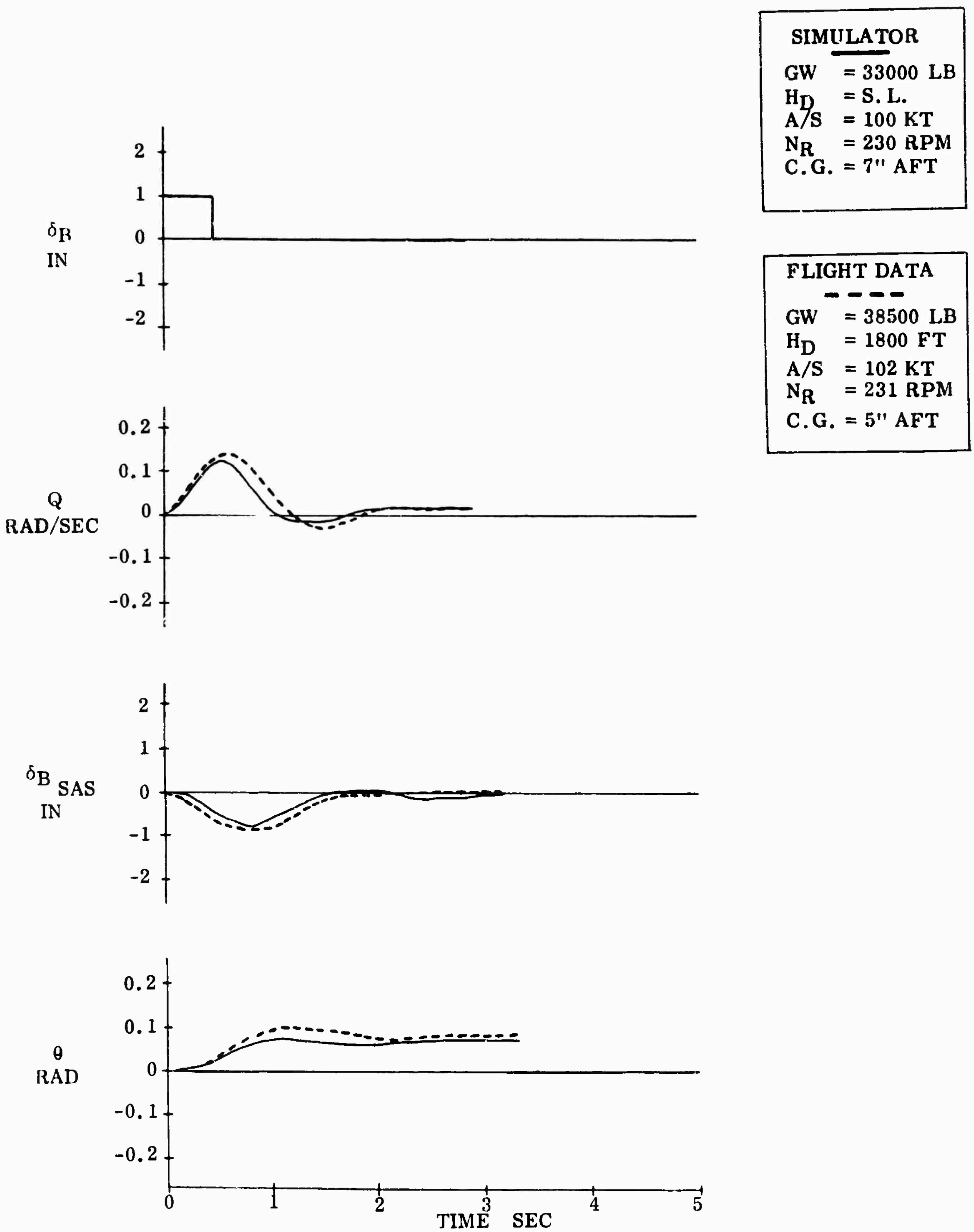


FIGURE 10

FLYING QUALITIES REQUIREMENTS

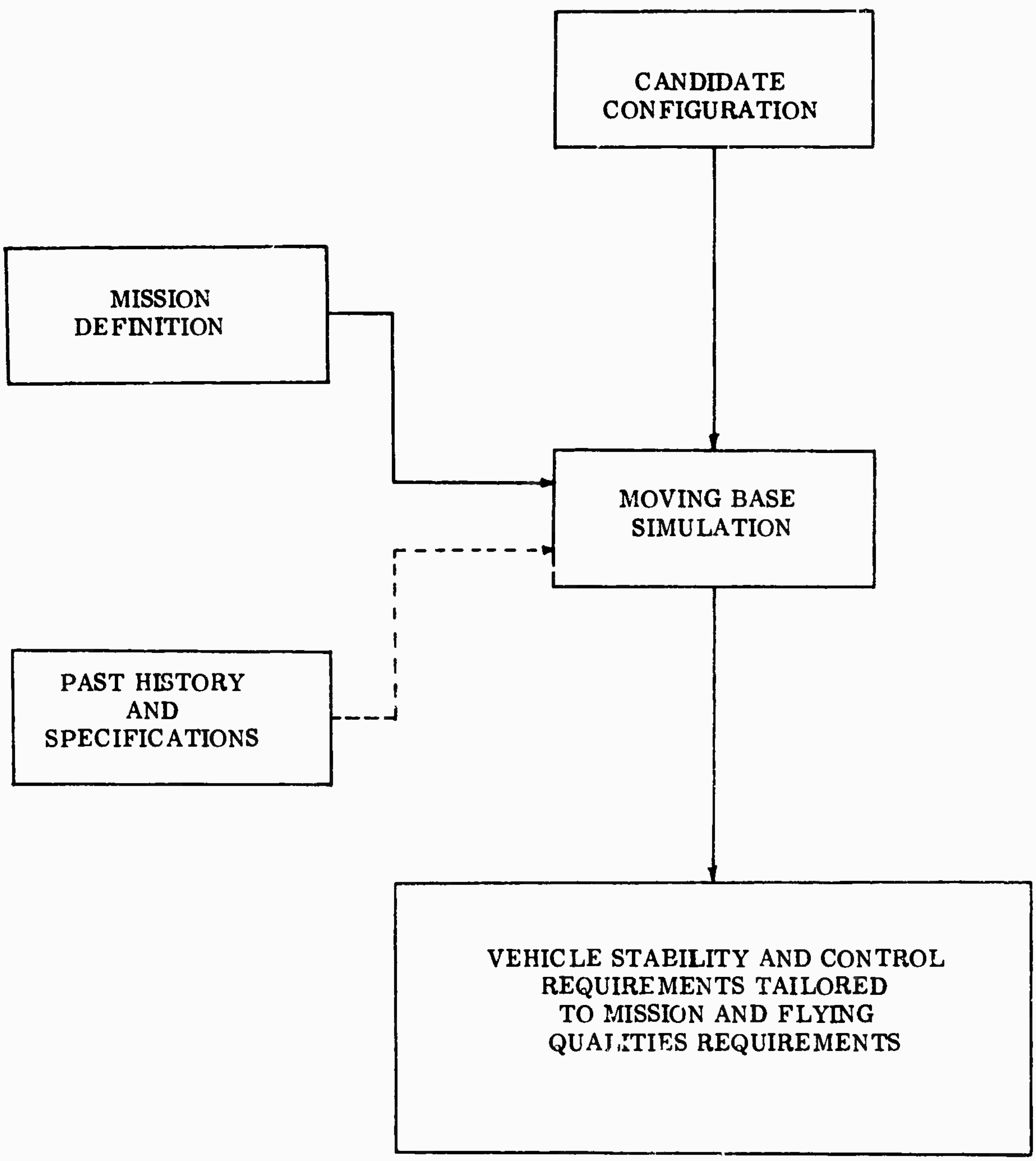
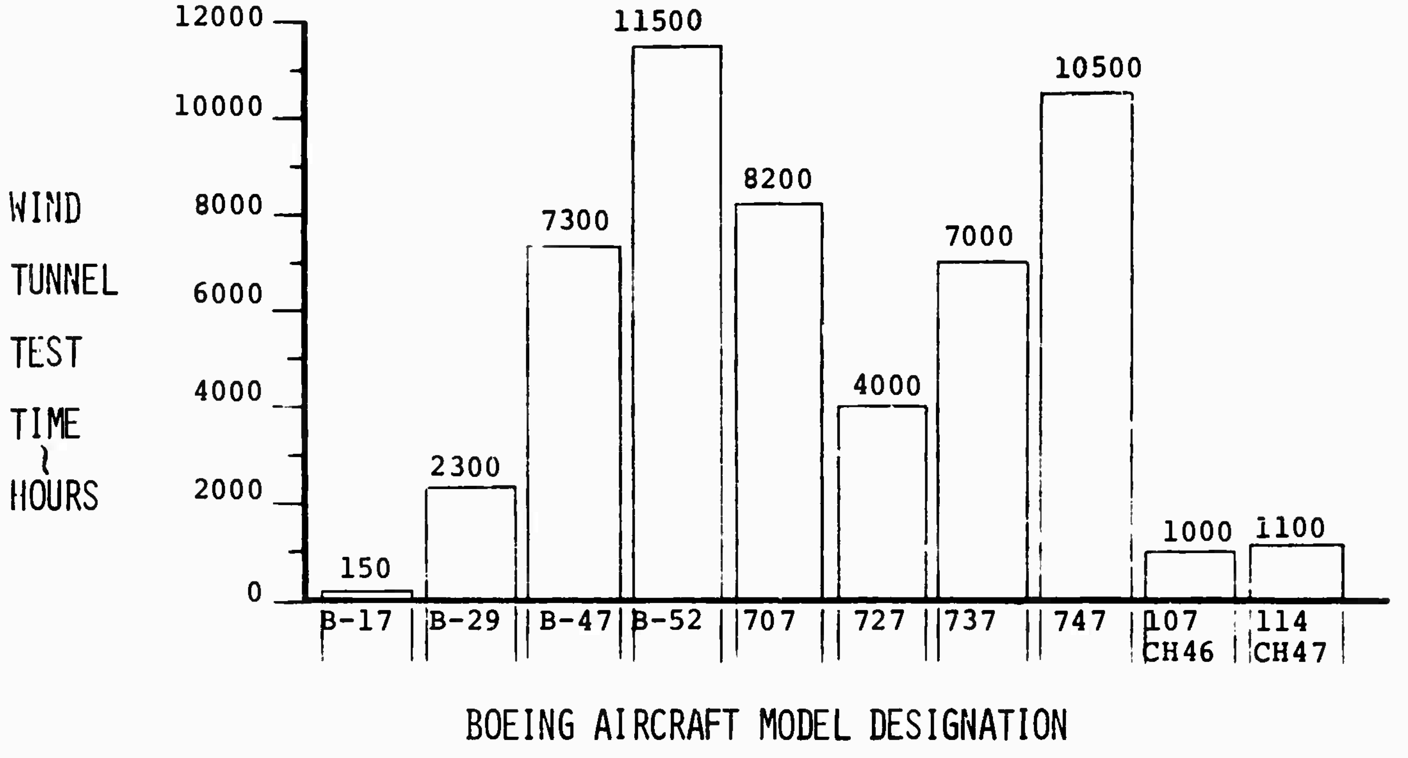


FIGURE 11

WIND TUNNEL TEST

HISTORY AND CAPABILITY



ROTARY WING REQUIREMENTS VS CAPABILITY

TEST REQUIREMENT	PROVEN CAPABILITY
ROTOR PERFORMANCE	FAIR
STABILITY & CONTROL	POOR
LOADS & VIBRATORY FORCES	FAIR/POOR
FLUTTER & DYNAMICS	FAIR
MODEL ECONOMICS	POOR
NOISE	POOR

NOTES ON ROTOR AERODYNAMIC RESEARCH

by
F. B. Gustafson

NASA Langley Research Center
Langley Station, Hampton, Va.

INTRODUCTION

The papers given at this meeting provide detailed excursions into many aspects of rotor and V/STOL aerodynamics. This panel paper will touch on three points, which are not closely interrelated but are chosen to add to the basis for discussion of future research action. The three topics thus chosen are: theory-data comparisons; rotor-blade flow visualization; and ways to use more blade-section camber.

THEORY-DATA COMPARISONS

The first point is simply the observation or opinion, based on consideration of rotor-aerodynamics efforts in general within recent years, that in a large percentage of studies there is a serious need for more thorough theory-data comparisons to show what can and cannot be done and what the significance is. The theoretician usually stops when he finds that there is experimental data which can more or less fit a case or two. The experimenter similarly appears, quite commonly, to be satisfied with a minimum of correlation with theory. Possibly this situation has something to do with the mechanism of modern approaches to organizing or contracting for research; in any event it seems to warrant a conscious effort toward improvement.

ROTOR-BLADE FLOW VISUALIZATION

The second point illustrates the importance of obtaining basic or overall understanding of flow phenomena. The problem in question is that of the gross aerodynamics on the retreating blade of a rotor when the local angles of attack are high. In many cases the blade develops far more lift, and has a very different value of drag, than would be obtained for static, two-dimensional flow conditions. Figure 1 illustrates some of the more recent data, cross-plotted from reference 1 and compared with theoretical values from reference 2. It is interesting to note that, as was the original intent with less advanced theory many years ago, the "stall limit" lines are actually limits of validity of the theory, even though stall and Mach number are now in the calculations. Both the lift for a given angle, and the power for a given lift are in this case much more favorable than predicted by theory. (Note: The horizontal (angle of attack) shift between theory and data below the stall limits has to be ascribed to experimental difficulties in setting angles; the slope differences above the stall limit are the item of significance here.) It is actually too easy to name plausible reasons for the slope differences shown; the question is, which reasons are primary, and how consistently so? There are, in this connection, many interesting efforts measurements at the blade surface. Also, many flow-visualization pictures are obtained from the point of view of the stationary observer - for example, from outside a wind tunnel. What seems completely lacking is flow visualization from the point of view of the "rotating observer," showing a cross section of flow going past the blade section. This is the type of picture which is automatically obtained for an airplane wing with a camera attached to it, but will require special effort to obtain for rotating blades.

An illustration of the potential value of such flow-field information is provided by recent work with highly swept airplane wings. Figure 2 shows, in the upper insert, the flow field around a delta wing as seen by an observer moving with the wing. Knowledge of the existence of this vortex flow, plus some ingenuity, led to the "suction analogy" theory which is seen to be in close agreement with the experimental force measurements. Theory assuming attached flow is seen to produce poor agreement. Similar comparisons exist for a considerable range of variables, and for drag as well as lift; more details may be found in references 3 and 4. While it is possible that the rotor-blade case may at times involve similar vortex effects, the real point here is that knowledge of the flow field, in depth and as seen by the lifting surface, can lead to remarkable progress in predictability at the higher angles of attack. It is suggested that steps are in order to obtain the equivalent flow-field information for the rotor blade, even though it is an unusually difficult problem in flow visualization.

WAYS TO USE MORE BLADE-SECTION CAMBER

The third and final point illustrates those possibilities for improved aerodynamic efficiency, on which a start can be made without waiting for the improved understanding just recommended.

Rotor blades have, for various reasons, done without the potential benefits of incorporating amounts and chordwise locations of camber which produce appreciable blade-section diving moments. Examination of current trends in design of new airfoils suggests that the importance of using such camber may increase greatly.

The primary obstacle seems to be the periodic, or vibratory, once-per-revolution variation in moment in forward flight, which can produce serious control-system fatigue loads. These variations reflect the product of varying dynamic pressure and a constant moment coefficient, producing a variation roughly as shown by the solid line in figure 3. If the moment coefficient can be caused to vary more or less inversely with the dynamic pressure, so that the product would tend to stay constant, it should then be feasible to use much more camber. A possible step in this direction is to use a flexible, or springy, trailing edge; it can be referred to as "self-adaptive" in that no linkages are required. With no airload, the camber is sizable; with increased "q" (dynamic pressure) the camber is reduced by the airload. The result is a different camber on the advancing blade (high q) and the retreating blade (low q) as sketched in figure 4. In turn, the tendency then is to produce a relatively constant moment as represented by the dashed line on figure 3.

This idea must be about as old as bird flight but perhaps now is the time to apply it. Perhaps the toughest problem is to get a springy trailing edge which will stand rough handling; there are also limitations to be imposed on the springiness to prevent unwanted aerodynamic effects during starting and stopping, for example. Thus, the percent of the ideal springiness which can be used would have to be determined for each specific design, and would also depend on the ingenuity used in offsetting potential problems.

As a more general point, regardless of the extent to which this "self-adaptive" principle can be used, there is still a need for more complete determination of the basis for identifying acceptable amounts of pitching moment for a variety of representative helicopter rotor designs; for example, in some cases the periodic moment may not be the whole story.

CONCLUDING REMARKS

To summarize, the points covered touched on the general approach to increased knowledge of rotor aerodynamics, a suggested tool for better basic understanding, and a sample means for specific improvement. Taken in that order, these points are:

1. As an opinion, and with exceptions, it is suggested that more thorough theory-data comparisons would pay off in rotor aerodynamics studies.

2. It is suggested that progress could be made on high tip-speed ratio, high section angle-of-attack rotor aerodynamic problems by devising and using a technique for getting flow visualization in cross sections around the retreating blade as seen by the "rotating observer."

3. Finally, it is suggested that more attention be given to determining how much blade-section camber can be used; and more specifically, that improved aerodynamic characteristics might be obtained by making the trailing edge "self-adaptive" (flexible) and thus permitting increased camber.

REFERENCES

1. McCloud, John L., III; Biggers, James C., and Stroub, Robert H.: An Investigation of Full-Scale Helicopter Rotors at High Advance Ratios and Advancing Tip Mach Numbers. NASA TN D-4632, July 1968.

2. Tanner, W. H.: Charts for Estimating Rotary Wing Performance in Hover and at High Forward Speeds. NASA CR-114, November 1964.

3. Polhamus, Edward C.: A Concept of the Vortex Lift of Sharp-Edge Delta Wings Based on a Leading-Edge-Suction Analogy. NASA TN D-3767, December 1966.

4. Polhamus, Edward C.: Application of the Leading-Edge-Suction Analogy of Vortex Lift to the Drag Due to Lift of Sharp-Edge Delta Wings. NASA TN D-4739, August 1968.

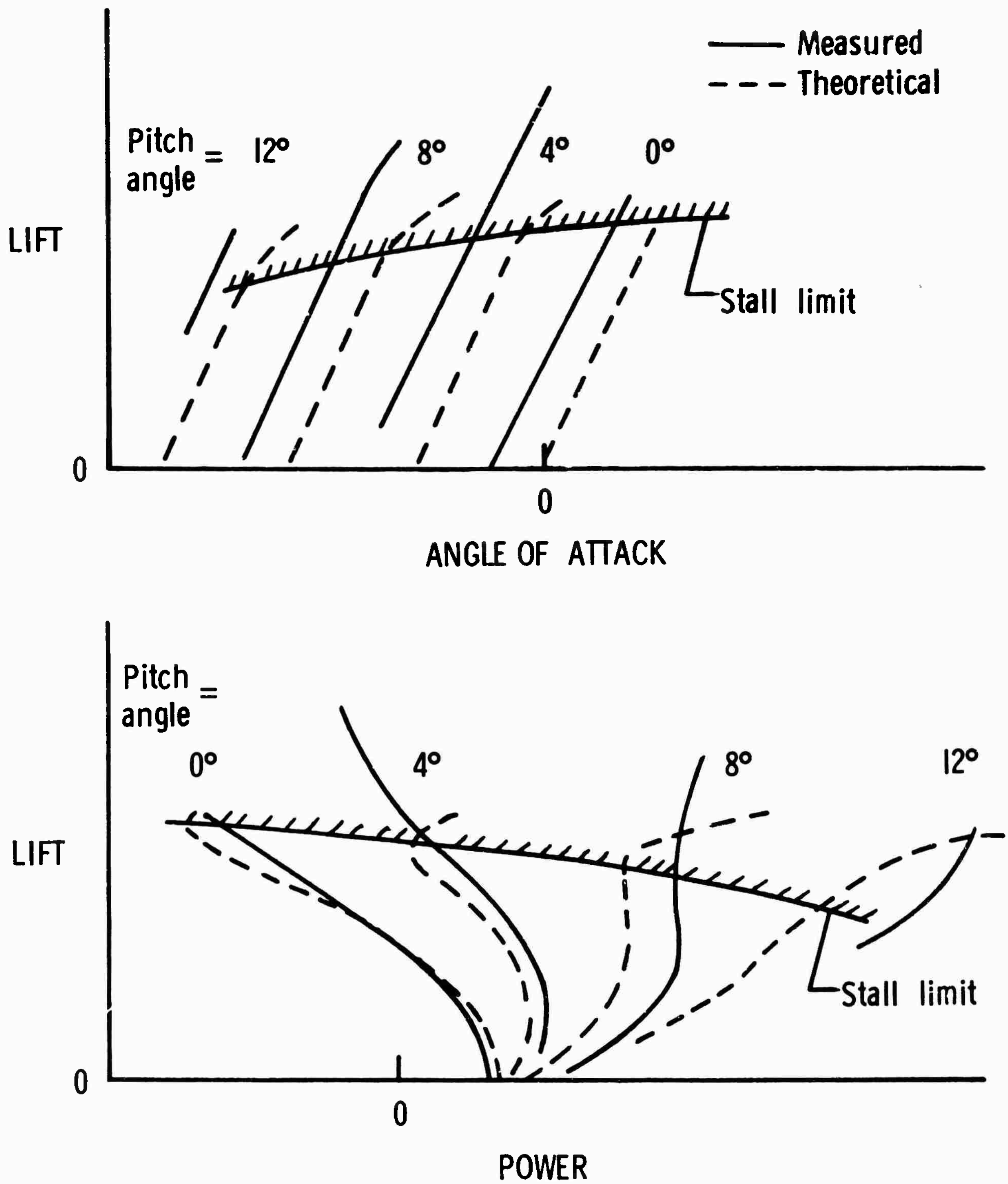


Figure 1.- Theory-data comparison at a tip-speed ratio of 0.5.

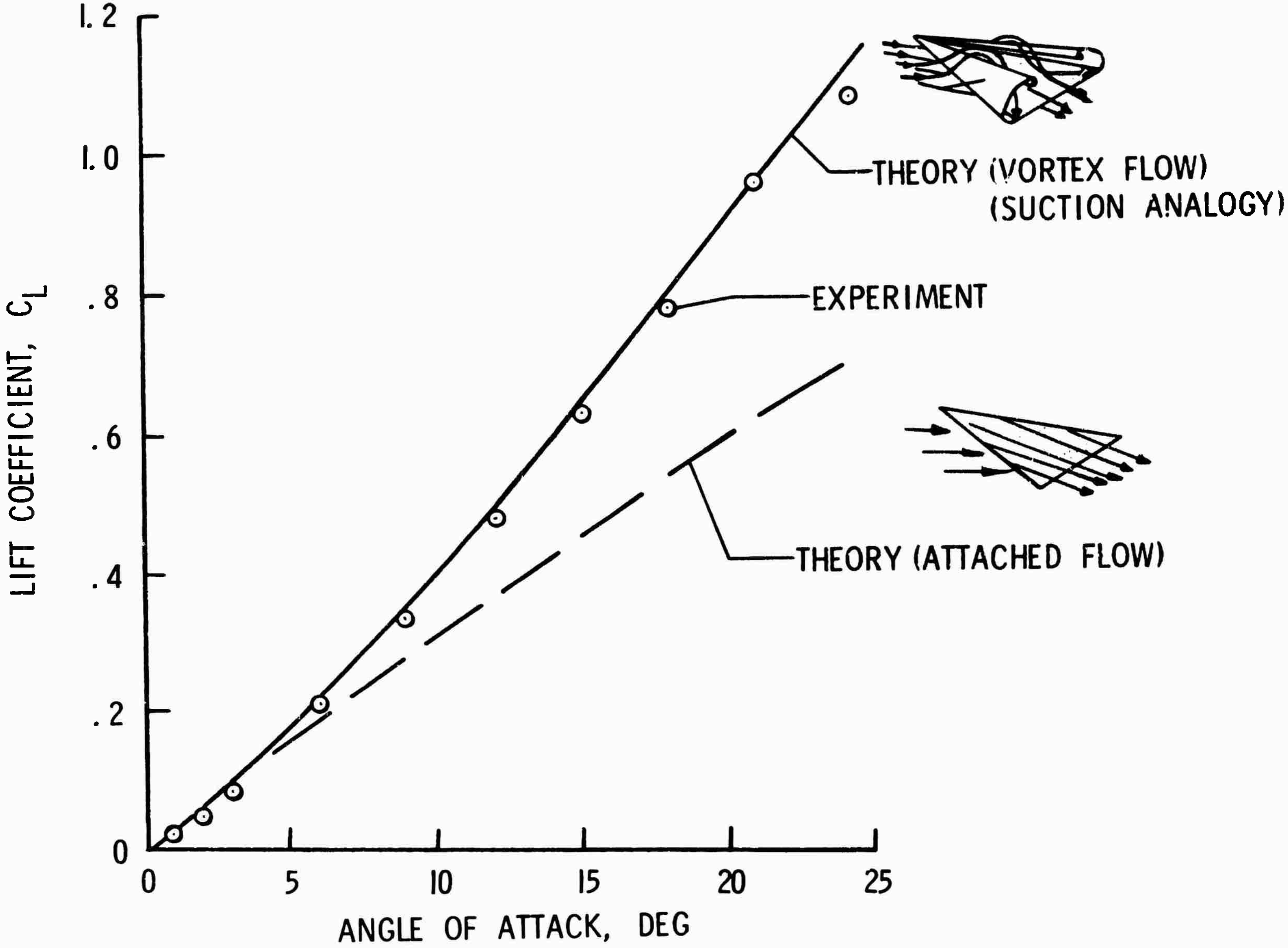


Figure 2.- Theory-data comparison for a sharp-edge delta wing.

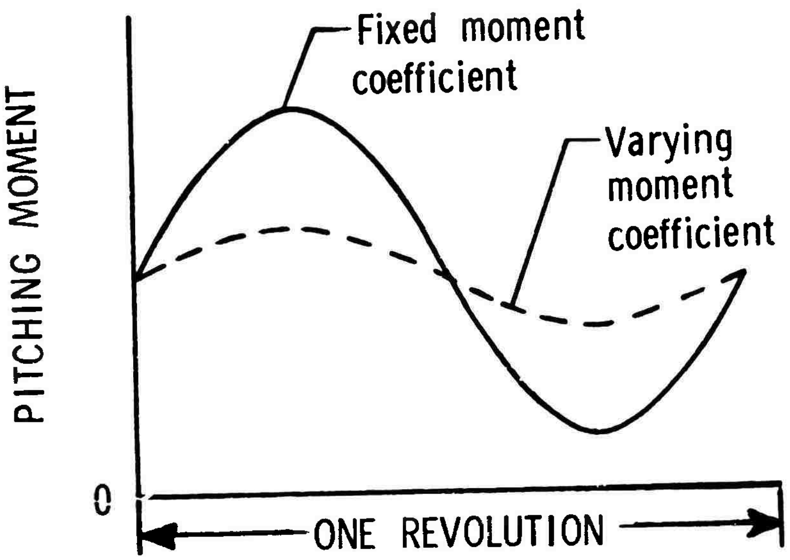


Figure 3.- Rotor blade-section pitching moment variation with blade azimuth position.

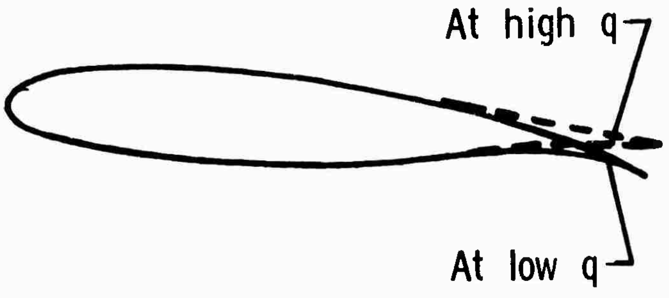


Figure 4.- "Self-adaptive" cambered airfoil cross-sections for advancing blade (high "q", or dynamic pressure) and retreating blade (low q).

SUGGESTIONS TOWARD A CO-ORDINATED APPROACH
TO
AERODYNAMIC RESEARCH

by

J. M. Harrison

LOCKHEED-CALIFORNIA COMPANY

Burbank, California

SUMMARY

A suggestion for a more co-ordinated treatment of aerodynamic research is outlined involving a two-pronged attack. A larger proportion of experimental work could be aimed at the acquisition of basic primary data, as against purely ad hoc measurement, and then applied using advanced analytical techniques made possible by recent gains in computing capabilities. Such techniques should be developed on a more universal basis with soft-ware packages designed around standard input/output formats.

INTRODUCTION

The ultimate objective of aerodynamic research aimed specifically at developing rotary wing and closely related V/STOL aircraft, should be to improve the overall performance potential of the class as a whole. To this end it is suggested that the conventional helicopter be treated as a reference datum and the factors inhibiting its performance be examined and classified.

Three main categories of limitation are cited below:

1. Aerodynamic limitations leading to
   (a) Excessive power demands.
   (b) Loss of lift and propulsive capability.
   (c) Saturation of control range.
2. Stability and handling qualities
3. Restrictions imposed by fatigue considerations

These three categories apply to all associated V/STOL vehicles. In order to extend the flight envelope, a two-pronged attack is suggested; first, thru acquisition and interpretation of primary data; second, the application of the processed data in analytical studies made to gain insight into, and to quantify the physical problems as they are encountered. The shopping list resulting from such an approach becomes formidable indeed, and as poor relations in the aerospace hierarchy, V/STOL specialists have to be selective and use as much by-product information as possible. As an example, the basic aerodynamics of the airframe is covered to some extent by conventional fixed wing research. With some persuasion, big brother can be persuaded to extend the scope of low speed aerodynamic work, probably under the guise of STOL applications. More effort could then be devoted to the more difficult problems associated with rotor performance, and aerodynamic interference between rotors and fixed surfaces, to say nothing of rotor/rotor interference.

Kind of Primary Data Needed

Looking at the three categories in more detail, power limitation is largely a question of drag reduction. As indicated above, effort should be concentrated on areas peculiar to V/STOL vehicles, the rotor itself and the interface with the airframe. Scope in the latter case could, with advantage, be widened to include research leading to efficient aerodynamic integration of lift, propulsive, and primary flight controls. In the case of the main rotor blades, all three sub-categories are closely related, since drag divergence is closely associated with stall. Then, probably the most common form of control saturation met with is traceable to divergence of the aerodynamic feathering moments. Category 1 calls for a large fund of reliable airfoil section

data. Examining stability and handling qualities, airfoil characteristics again emerge as a key. Computing the response of the rotor to external disturbances requires knowledge of section performance over a wide spectrum. The same can be said about the calculation of realistic loading actions, a prerequisite to estimating oscillatory stresses, and thereby component lives. The need for comprehensive 2D-airfoil data, steady and unsteady, of a representative range of applicable airfoil sections is widely recognized, and there is a steady flow of information from a variety of sources. Uncritical acquisition of data is however not enough, and the technique of measurement should include provision for visualizing limiting mechanisms, thereby facilitating the identification and selection of optimum sections. Investigation then requires extension to include 3-D effects. There is a strong case here for integrating both experimental and analytical programs with parallel research devoted to fixed wing applications, in fact the same teams could be responsible for both, organized at national level, and with international cooperation. Sufficient knowledge and experience has been acquired by now to realize that conventional airfoils are restricted in typical helicopter configuration even when great effort is expended on optimizing blade geometry, for even the best is but a compromise. The time is ripe to switch more resources to advanced concepts such as circulation controlled airfoils, with the added incentive that they provide alternative potential means of controlling the aircraft.

Advanced rotary wing designs using fixed lifting surfaces are shortly due to make an appearance and another whole field of research is being opened up. For such categories of rotary wing aircraft, compound helicopters and tilt wing/rotor vehicles, to achieve optimum performance over the whole flight envelope, there must be careful harmonization of all the main aerodynamic components. Extensive use of the new generation of low speed V/STOL tunnels requires funding not only to carry out ad hoc testing of entire vehicle models, but also to generate more basic data. Performance of typical rotors, isolated and in wing/rotor and wing/rotor/body combinations, with and without high lift and additional trimming devices, should be investigated using idealized models. Investigation should cover a representative range of flight conditions, with systematic variation of all geometric parameters identified as being of importance. It is moreover, not sufficient to measure only steady data. In

particular, little attention in the past has been given to close passage between rotor blades and fixed surfaces a potent source of frequency excitation. Yet, another aspect is coming into prominence, and it is necessary to weigh the cost of providing dynamically similar models against the ability to investigate in a reasonable degree of safety, potentially unstable modes, which are being encountered more frequently as flight envelopes are extended.

Analytical Approach

Without making any serious attempts to exhaust the shopping list, examine now the second prong of the suggested line of attack. The proposal is to make a systemized analytical study of problems as they are encountered or, more hopefully, anticipated. There are interfaces here with other specialities, but the aerodynamicist has an important part to play. Again three main categories are involved, of mathmodel in this instance:

1. A predominantly analytical tool, a related series of multi-degree-of-freedom models, simulating combinations ranging from an isolated rotor to the entire vehicle, and sacrificing real time capability in order to achieve realistic representation. Programmed for digital computer.

2. A flight simulation model bred from 1 and degraded but only to the extent that real time performance is possible. Programmed for hybrid or analog computer.

3. A small perturbation version of 2 programmed for analog computer.

It is assumed that all leading organizations have an assortment of such tools, and that their application is similar. No. 1 comprises the main artillery for evaluating rotor and overall vehicle performance, in terms of lifting and propulsive capability, and power requirements. Such tools can however be made sufficiently universal to deal with problems in all three categories. Thus, loading actions along the blade span and at the hub are an

important by product, enabling limitations under categories 1 and 3 to be identified. Blade loading actions can also be applied to the investigation of vortex noise generation. With sufficient degrees-of-freedom admitted, it is, in addition possible to investigate the stability of the vehicle and sub-systems over a frequency spectrum ranging from zero, through sub-harmonic to high harmonic. This type of investigation is however, usually delegated to model 2, particularly if it is required that the loop be closed around a control system including real hardware and/or a human link. No 3 is a simpler but nevertheless important member of the family supplementing 1 and 2. A typical application would be in the early stages of investigating novel control systems. The application of such programs ranges well beyond the scope of this symposium and need not be dwelt on. The role of the aerodynamicist includes the supply of aerodynamic packages, three in number. Important gains could be made if there was some attempt to devise a universal approach or, at least, standardize, on a number of alternatives. Thus, it could presently be assumed say, that the program module computing and resolving generalized forces acting on the blade, would convert blade pitch, along with incident velocity and induced flow vectors, to section Mach number and angle-of-attack in the range 0-360 deg., before transferring control to the airfoil data package. This package, containing data in synthesized quasi-2D form, would output coefficient of lift,drag and pitching moment, transferring control back to the main program which would then proceed to compute the blade loading actions. The third package also in the loop, accepts the loading distribution in suitable form and computes a downwash distribution in the rotor tip path plane. The concept hinted at is a standard communicating format enabling a variety of airfoil data and downwash sub-routines to be plugged in at will to the main rotor segment. As the state-of-the art advanced, the main rotor module might be developed to incorporate more powerful methods, and the formats changed to admit extra parameters. Such an approach would call for the establishment of an authority to prescribe the standards and would require the acceptance of that

authority. Software investment is however sufficiently large to make the effort worthy of consideration. There might be a case for funding joint ventures in which an academic or research institution were to devise in consultation with an industrial partner, an advanced analytical method to some agreed specification, leaving that partner to adapt the resulting method to engineering applications, and then for both to cooperate in the verification against experimental, preferably full-scale data.

Particular Applications of Analytical Methods

The approach as outlined may have been restricted in the past, but rapid strides made in hardware and software development, renders economical the tackling of problems considered hitherto hopeless. Thus the vast amount of effort put into developing performance methods for isolated rotors in axisymmetric flow might be extended to cover rotor/lifting surface, and rotor/rotor combinations. Which conventional helicopter has not had tail rotor problems? Further extensions to deal with interference in asymmetric flow begin to look feasible, but it is doubtful if the investment in a number of widely different methods could be justified economically. There is as great a shortage of good men as of money.

Closing on a controversial note, one fruitful competitive field which has not been entirely neglected, concerns the best way of achieving high subsonic speed in a vehicle with V/STOL capability. It is generally accepted that the compound helicopter is probably limited to some 300 kts, the tilt wing/rotor vehicle to 400 kts. To attain higher speeds, the rotor must be temporarily disposed of. One school of thought favors an extension of the tilt rotor concept, stowing the blades umbrella fashion before transition to the high speed band. The other school of thought favors stopping the rotor in the horizontal plane with the option of stowing it. The variations on these two themes make for interesting and demanding work. Although the vertical stoppers appear, currently to have the edge, the tolerance for extreme configurations, of symmetrical elliptical airfoils with circulation control, could change the picture within the near future. The best way of achieving high speeds consistent with V/STOL capability has yet to be resolved, but an attractive bonus to be earned from the endeavors is a further massive gain in analytical capabilities.

FUTURE AERODYNAMICS RESEARCH

Robert R. Lynn
Chief of Research and Development
Bell Helicopter Company
Fort Worth, Texas

During the panel session on Dynamics Loads Problems at the first CAL/AVLABS Symposium in 1963, a list of recommended aerodynamics research items was presented. The list included:

- Improvements in analytical techniques with respect to induced velocity, boundary layer and radial flow
- New airfoil development
- Test of existing airfoils at proper Rn and Mn

In 1965 the results of an industry-wide survey on rotorcraft research needs were submitted to the NASA Research Advisory Committee on Aircraft Aerodynamics. The resulting recommendations added the following items:

- Definition of rotorcraft limits (e.g., efficiency; advance ratio; Mach number; rotor loading)
- Component drag reduction
- Development of test techniques and instrumentation
- Correlation of analyses, and small and large scale tests
- Interference effects (e.g., rotor, propeller, wing, etc.)

Several other items of "lesser" importance were also noted. One of these was noise.

Even though the emphasis has shifted somewhat, and the specific details have changed due to the added visibility that the intervening years have given us, these lists can serve today as general guides to needed aerodynamics research. In fact, they make a good summary of the topics discussed during this symposium. So these listings must be doing some good. People are working on the previously recommended research and progress is being made. Monies have been made available for the work and its reporting. This is the major value of sessions such as this - - - that is, program justification. Today, we are helping to justify our future aerodynamics research programs.

In this presentation, I will attempt to indicate my beliefs as to the direction that future aerodynamics research should take and provide some reasons why the work is desirable. No new list is given. It is hoped that the recommended items, or areas, are apparent as we go into the future - - - and look back.

Setting

Imagine that it is the year 1979 and we are at the eighth CAL/AVLABS Symposium. The theme of the meeting is, "A Review of the Past Decade of Aerodynamics Research in VTOL."

The general situation is that the helicopter, as a type, still predominates in the VTOL field. Large helicopters make use of some compounding. The tilt proprotor aircraft is just entering commercial service. A 60,000 lb. folding proprotor "technology" aircraft has recently achieved a flight Mach number of .963 at its design altitude of 35,000 feet. A small tilt wing aircraft is in use in the military service and has been for several years; a study is underway for a 300-troop transport version of such a machine. A competition for a 2nd generation jet VTOL fighter has just been completed, and a VTOL version of an SST is under study. Now - What was discussed at the meeting? And, What is the background? My job is to summarize, for selected areas, the state-of-the-art and background as indicated during this five-day meeting.

State-of-the-Art Summary (1979) and Background

Wake; Induced Velocity - The theoretical techniques which define the rotor* wake, developed during the latter part of the 60's and early 70's, are being used routinely in current engineering design - - - but not for the purpose they were originally developed. These analytical tools have been used to (1) define and minimize the effective download of fuselage-wing-rotor combinations (effective download losses, defined as the difference between the download and the increased thrust due to download, have been reduced to one-third of their former values); (2) nearly double the swath width for agricultural helicopters; (3) increase the capability of the municipal heliborne firefighting systems in combating congested area fires by using the rotor downwash more effectively to carry the fire fighting chemical; and, (4) define the aerodynamic environment of tail rotors and the interference between rotors operating in close proximity; thus, reducing tail rotor power required and eliminating most of its control and structural loading problems. The importance of these theoretical techniques and what it is possible to do with them cannot be overemphasized.

These techniques, which involve the rotor flow field were developed originally to predict local inflow velocity at the rotor blade so that higher frequency structural loads and hovering performance could be calculated more accurately. The flow field approach was abandoned when it was shown experimentally that although the theory provided acceptable predictions of the characteristics of the remote wake, the discontinuity of the flow field caused by the rotor blade itself, for many cases, invalidated the calculated induced velocity at the blade.

*The term rotor is used to include the helicopter type rotor, proprotor and propeller.

As you heard today, at the present time the tangential, radial and normal wake flow on the top and bottom surfaces of the blade are defined by superposition of a simplified flow field representation and tip vortex effects. The vortex strength and path are determined empirically as a function of tip shape, disc loading, blade loading, tip airfoil section, and Reynolds and Mach numbers. It has been shown experimentally that this approach adequately defines the total effects of the wake at the blade, within the range of presently available experimental data. It was noted during the meeting that additional data are needed, and that efforts should be renewed to solve the problem analytically.

The semi-empirical approach to calculating the induced velocity and the effective airfoil shape due to the wake radial flow component is practical only because of the computer storage sharing facilities now operated on the east and west coast by NASA. With the opening of the new facility at the Langley Research Center last year, nearly every university and manufacturer has access to virtually unlimited data storage capacity.

Noise - The development of the semi-empirical means of handling the tip vortex and rotor wake has resulted in reductions in the rotor noise that once plagued the industry. Although the rotor blade slap and tail rotor rotational noise can still be heard when some of the older helicopters fly, maximum sound pressure levels at 200 ft. for most new machines are in the 80 to 90 db range. This is a reduction in noise level even with the increased power loading now used. Not surprising, a significant reduction in power, both in hover and forward flight, accompanies the noise reduction.

As with most physical phenomena, the analytical-empirical definition of the adverse tip vortex effects was preceded by an interesting experimental approach, with which the major gains in noise reduction were made. This was prior to the development of an understanding of the related phenomena. Noise and tip vortex strength measurements were made for small rotor models. The models' blade-vortex interaction and rotational noise were converted to equivalent full scale values based on the relative tip vortex strength of the model and full scale rotors. Large numbers of tip shapes and airfoil sections were evaluated. Only the most promising were investigated in full scale. By the early 1970's the vortex-blade interaction noise, as well as the compressibility-induced blade slap, was essentially eliminated.

Instrumentation; 3D Boundary Layer - The gains in hover and high speed rotor efficiency and the reductions in noise were preceded by marked improvements in the techniques and instrumentation used to define the wake and boundary layer effects. The principal tools of the experimentalists, developed initially during the late 60's, were the rotating blade Schlieren and blade angle-of-attack, acceleration, air velocity and pressure sensors.

The latest advancements in instrumentation allow the measurement of induced and profile drag on a rotating blade. With the increased computer storage capacity now available, it may become possible to work directly with rotating airfoil data, instead of the presently used 2D data. Whether or not this will become economical remains to be seen.

With the early instrumentation equipment, once considered so advanced, the experimenters developed a limited understanding of the 3D boundary layer in sub- and transonic flow, in- and out- of a centrifugal force field. The payoff was high. Even with a limited knowledge of the viscous effects and how to control them, airfoil skin friction drag was reduced some 50% and profile drag, slightly less.

Currently, the boundary layer techniques are being used to predict analytically the time variant aerodynamic characteristics of an airfoil near stall. In the late 60's a considerable amount of test work was accomplished to define these effects. The objective then was to explain the origin of the apparent high values of blade lift and moment coefficients. This work stopped later when it was found that if the true local velocities were used, reasonable values of the coefficients were obtained. This work has been initiated again analytically to investigate the possibility of designing to use the higher dynamic lift coefficients for performance gains and using the lift hysteresis to extend certain types of rotor instability thresholds.

Airfoil Development; High Lift Devices - Airfoil sections introduced in the early 70's provided the designer with some 50 to 60% more usable lift coefficient than previously available. Also during that time, special blade tip airfoils, developed in a transonic tunnel, allowed the efficient penetration of the sonic regime for off-design conditions. The high-lift airfoils resulted in a greatly reduced blade area needed to meet the maneuver and control criteria, which were established in 1970. A major hovering improvement resulted. Significant weight savings were achieved due to the higher tip speed, lower torque designs made possible by the sonic airfoils.

The principal reason that so much progress was made in the first half of this decade is the strong support given the airfoil development programs throughout the country. Also to be credited, are analytical tools which allowed airfoil development by wind tunnel simulation techniques. Only the final refinement of new airfoils and final data tests needed to be made in a wind tunnel. During the past several years there has been a plateau in airfoil development, similar to the one in the 50's and 60's.

Since about 1975 a great deal of attention has been directed toward various high lift devices. Moving flaps and circulation control have been used successfully on the inboard

sections of rotor blades and hub to improve efficiency. However, to date, the efficiency and simplicity of the basic airfoil has predominated. During the meeting, additional airfoil and high lift devices development were noted to be needed.

Component and Interference Drag - The total drag of new rotorcraft, especially the helicopter and proprotor, has been reduced significantly. The high pylon and hub drag associated with the helicopter is now a fraction of its former value. Hub and pylon boundary layer control, systematic wind tunnel investigation of rotorcraft components, and the avoidance of interference drag are credited with these gains.

Efficient engine inlet and exhaust systems, including particle separators, are in common use. These, too, have resulted from extensive wind tunnel development programs conducted during the procurement of several new aircraft. In a paper given at this meeting it was pointed out that the gains in overall efficiency resulting from proper engine inlet design could provide up to 30% payload increments for critical missions.

Mach Number-Advance Ratio Correlation - During the meeting it was noted that good correlation exists between theory and small and large scale tests with respect to power, control angles and gross aerodynamics parameters up to advance ratios of .75 and tip Mach numbers up to 1.3. These values are somewhat higher than those used for current production machines. It was shown conclusively in the early part of the decade that with special blade tips and airfoil sections, high Mach number operation is preferable to operation at high advance ratio. Rotor stability problems at high μ, of course, have deterred penetration of this area.

The constant improvement in the technology of rotor aerodynamics has resulted, to a large extent, from the many test programs which have been conducted in the NASA-Ames full scale wind tunnel over the past 15 years. Major progress has been made during the past five years since that tunnel was rebuilt to achieve a speed of 300 knots with a somewhat larger test section. This was accomplished because, even in the late 60's, research helicopters outstripped the tunnel capability, and higher test speeds and a larger section were needed to develop other VTOL types. The United States again has the largest, fastest tunnel in the world with which to develop VTOL aircraft.

NASA was commended during this meeting for its far-sightedness in rebuilding this tunnel. That this refurbishing is in the national interest is proved by the technological lead in VTOL that this country now enjoys; for example, the 60-passenger tilt proprotor commercial aircraft scheduled for passenger service in fourteen countries at the year's end. The tunnel modernization, which is considered to be a masterpiece of equipment updating, was accomplished with only six months tunnel down time. Sections of the tunnel were prefabricated and set in-place by heavy lift helicopters.

Manufacturing Techniques - With the advanced instrumentation and the subsequent increased understanding of the viscous effects, the requirement for smooth, close tolerance blades became even more obvious and the design, manufacturing and inspection teams turned their attention to these items. This led to techniques which resulted in significantly increased performance and interchangeable blades without whirl or flight test. The major innovations, still in use, are (1) the female cavity tool for blade construction (now there is no slippage in the autoclave and near-perfect contours are achieved); and, (2) the use of wear-resistant materials for blade surfaces, and improved paint and surface protection systems.

Because of these and other innovations, laminar flow is maintained over 90% of the lower blade surface and 30% of the upper. Blade drag coefficients are reduced some 30%. Additionally, the use of these new techniques brings us closer to the blade service life goal of 10,000 hours.

Epilogue

Having introduced the thought of a 10,000-hour service life for blades and having rebuilt the Ames 40 x 80-foot tunnel (or should I say, 60 x 120-foot), let us come back to the present. - - - It is now June 20, 1969.

Fantasy? Not entirely. These items, or at least general areas, should be investigated. What do you foresee? How do you relate what you see to our present and future needs? Your answers to these questions will be our future aerodynamics research. Let us hope that today we justify these programs well.

AERODYNAMIC RESEARCH RELATED TO PROPELLER DRIVEN V/STOL AIRCRAFT

O. E. Michaelsen
CANADAIR LIMITED
SUBSIDIARY OF GENERAL DYNAMICS CORPORATION

AERODYNAMIC RESEARCH RELATED TO PROPELLER DRIVEN V/STOL AIRCRAFT

by

O.E. MICHAELSEN

Canadair Limited
Subsidiary of General Dynamics Corporation
Montreal, P.Q.
Canada

INTRODUCTION

Of the 18 excellent papers delivered here at the third CAL/AVLABS Symposium, 13 have dealt with the aerodynamics of helicopter rotors; 4 can be labelled general V/STOL, although two of these were on propellers and the other two on wind tunnel interference problems; and 1 paper was given on the hydrodynamics of ship propellers. While some of the previous speakers on this Panel have dealt with V/STOL on a broader basis than rotorcraft, one might consider that I enter enemy territory with recommendations for research related specifically to the propeller driven V/STOL aircraft.

PROPELLER RESEARCH

In an attempt not to disrupt the continuity of the proceedings, I would like to start by discussing research related to V/STOL propellers. For concepts employing propellers for both lift sustention in hover and propulsion in cruise flight, it would appear, a priori, that high levels of performance in both regimes of flight are necessary to produce an efficient vehicle.

However, one percent improvement in total static thrust is identical to one percent increase in gross aircraft hover weight while one percent improvement in cruise propulsive efficiency is only equivalent to about 1/2 to 3/4 percent reduction in cruise fuel required. Since the total fuel load typically is about 15 percent of the aircraft hover weight, the one percent improvement in propulsive efficiency is equivalent to about one-tenth of one percent hover weight. In other words, the exchange ratio between hover and propulsive efficiencies is about 1 to 10.

In addition to this obvious importance of static thrust efficiency, the propeller selection problem is complicated by the use of differential thrust for roll control in hover, and roll and yaw control in cransition, as well as the possible use of blade cyclic pitch for pitch control of the aircraft. Thus, we require a propeller which has an extremely high figure of merit at a high loading in order to keep the activity factor, weight and cruise efficiency within reasonable values. In addition, the figure of merit curve must be as flat as possible with increased loading such that the power increases due to differential thrust and cyclic pitch are kept low.

Until recently, the aerodynamics of the propeller in the static case was not well understood. However, a great deal of work has been done in the last six to eight years and we appear presently to make considerable progress, at least experimentally. This progress has been the result of employing several experimental techniques as well as some analytical ones. We have learned how to measure static performance accurately and repeatedly under closely controlled conditions so that changes in performance with changes in blade design can be established accurately. Techniques for measuring both the average and the instantaneous flow field around the propeller have been established and from these flow surveys it has been possible to obtain at least the mean angle-of-attack distribution for the blade. By use of estimated, or "guessestimated", two-dimensional airfoil section characteristics and simple strip analysis, the radial loadings can be obtained with reasonable accuracy. By analyzing the results, the required changes in twist distribution, camber distribution and perhaps planform to improve the static performance can be specified. Unfortunately, since the effects of such changes on the induced velocity distribution presently cannot be predicted, improving the propeller static performance is an iterative experimental process requiring considerable time and effort.

The possible results of such efforts are demonstrated in Figure 1, where the performance of several designs is shown in terms of figure of merit versus a "normalized" power coefficient to take account of the differences in activity factor of the various designs.

In this figure, the Curtiss-Wright 732 propeller design dates back to 1946 and indicates the best figure of merit of any propeller tested at Wright Field prior to 1960.

The XC-142 model propeller represents the developed blade and was furnished by Hamilton Standard for tests on the Canadair rig as part of Hamilton Standards static thrust research program. This propeller represents the best V/STOL propeller currently in use.

The CL-1967 and CL-1968 represent recent Canadair designs. It will be noted that these designs have figures of merit substantially higher than obtained with the best current V/STOL propellers. In addition, as shown particularly for the CL-1968 propeller, it has also been found possible to maintain good static thrust performance at higher blade loadings.

In addition to the above techniques, various forms of flow visualization have been developed. An excellent example of such a technique was given here early in the week. At Canadair, we have been using both continuous and strobed lighting of smoke jets, but have found the results more entertaining than useful so far although it certainly gives us a better qualitative understanding of the vortex wake structure. Blade surface flow visualization by use of sublimation of an organic solid has indicated that the transition line between laminar

and turbulent boundary layer flow is very far back on the airfoil section at light to intermediate blade loadings. Whether or not the line we see really represents transition is not certain - all we know for sure is that a forward shift in the line is related directly to a decrease in performance.

While some progress has been made, much more effort is needed to enable rapid and reliable aerodynamic design of efficient V/STOL propellers. It may appear redundant to point out the desperate need for airfoil section data again - this plea has been made by many authors already - but the data needed for propellers are quite different from that needed for rotors. V/STOL propellers tend to employ sections with extremely high design lift coefficient at a large variety of thickness-chord ratios and the section pitching moment characteristics are of secondary importance. Data are needed for a variety of Reynold's numbers, Mach numbers and surface roughness conditions. It is believed that the right approach is to start with analytical investigations, similar to those described by Professor Wortmann and Mr. Drees earlier in the week, including the effects of boundary layers and compressibility. Once suitable sections have been specified, experimental data must be obtained.

It is not known how serious the three-dimensional effects are near the propeller tip, nor do we know how the blade boundary layers behave or what effect they have on the

performance. The effective inflow as seen by the propeller blade, rather than as seen in stationary coordinates, and relating to the use of two-dimensional characteristics and strip analysis, is not known. Blade surface pressure measurements and boundary layer measurements on rotating propellers are needed to correlate with the present flow surveys and two-dimensional strip analyses.

An analytical method for determining the effects of changes to the propeller blade on the induced inflow is needed to eliminate the need for the tedious experimental iteration. It is believed that the "free wake" analysis is not likely to prove fruitful for at least several years and that work in the immediate future should concentrate on the "prescribed wake" approach. This approach is similar to that described by Messrs. Rorke and Wells of Sikorsky. Since it is likely that the prescribed wake will have to be different for each propeller, we must learn how to use the information from flow visualization to prescribe an adequate wake for each basic propeller design, as well as establish the sensitivity of the calculations to changes in the various parameters we prescribe.

For the conventional cruise case, we must examine the adequacy of the present strip analyses methods for dealing with the extreme twists and cambers that result from optimizing the propeller for static thrust. To my knowledge, all current methods are restricted to near-optimum load distribution, a situation

which is unlikely to be achieved for the V/STOL propeller in cruise.

Aerodynamic noise is an important aspect of the V/STOL propeller, since propellers generally tend to be even more noisy than rotors. Based on evidence to date, improvements in propeller static performance does not appear to influence the noise level significantly in either direction. Reduction of noise level without impairing the static performance seriously may prove an impossible task but certainly should be investigated. At present, virtually no data is available. It is felt beyond the scope of this discussion to recommend research related specifically to cyclic propellers since the optimization of such a propeller is closely related to the overall characteristics of the vehicle.

AIRFRAME RESEARCH

With regard to the characteristics of deflected slipstream, tilt wing configurations, systematic testing related to the deceleration-descent capability of such configurations are required to establish the trade-off between transition requirements and cruise economy and speed. A large amount of data has been generated, but most of the data is on specific, unrelated configurations under non-representative operating conditions with regard to Reynolds number, Mach number, disk loading etc. The appropriate relationships between chord-

diameter ratio, wing span extension and number of propellers need to be established for various levels of sophistication of the flap system. Work at Langley in the past few years has aimed at this objective, but much remains to be done.

Additional information on the effects of ground proximity on performance, trim, stability and control of propeller driven V/STOL configurations is also required, but it is difficult to recommend general research in this area since ground effects are strongly influenced by the particulars of the configuration.

While the lateral-directional low speed control problems can be solved adequately for tilt wing configurations, as evidenced by the CL-84 prototype aircraft, by virtue of a powerful control system, several more-conventional STOL aircraft have exhibited problems in this area. At least part of this problem is due to the fact that high-lift development has not been accompanied by a similar development in control devices. It would appear appropriate that research leading to the development of such devices be undertaken.

HANDLING QUALITIES

The need for exceptionally good handling qualities in V/STOL aircraft should be obvious if for no other reason than that they possess large installed power reserves over a large regime of flight speeds and, as such, have excellent maneuverability potential. The control powers required for tilt wing aircraft in the class of the CL-84 and XC-142 have been reasonably well established. In the case of the CL-84, the required pitch and roll control powers are considerably in excess of the minima specified in any of the current military specifications. Some control power information is also available from tests of STOL aircraft. However, no information is available on the control powers required in very large V/STOL aircraft.

The basic effects of aircraft size can perhaps best be appreciated by listing the variations in the pertinent parameters as a typical aircraft is scaled dimensionally. For V/STOL aircraft, the propeller disk loading and the wing loading do not tend to scale with size, i.e. the thrust to power ratio must be maintained within reason as the aircraft gets large. As a result, it appears reasonable to demonstrate how a cyclic propeller pitch control version of the CL-84 prototype scales with size under the assumptions of constant loadings and weights distributions. Strict adherence to the scaling rules do not change the non-dimensional numbers such as:

the thrust vector off-set for cyclic pitch control as a fraction of propeller radius; differential thrust for roll control as a percentage of hover weight, and so on. However, the control power in terms of angular acceleration is reduced inversly proportional to the scale factor.

If a constant control power, in terms of angular acceleration, is postulated, the scaling results in the pertinent numbers shown in figure 2. The reference control powers for the CL-84 are those established by flight test for the prototype as the minimum satisfactory values.

It is evident from the figure that the requirements for constant control powers, regardless of aircraft size, becomes ridiculous long before the aircraft weight reaches 100,000 pounds. Even if only one half the listed control powers had been considered, the hover performance penalties would have been enormous for the larger aircraft.

While the actual situation is not quite as adverse as indicated by these simple scaling rules, the exercise indicates that a real problem may exist in large V/STOL aircraft in matching the required control powers with those that can reasonably be achieved. Thus, any agency contemplating the future use of large V/STOL aircraft should sponsor work aimed at establishing the levels of control power required for such aircraft rather than applying the experience from small aircraft to the large ones.

DOWNWASH

This particular issue continues to be a controversial one. It should be evident at this time that only actual operational experience will resolve whether or not the problems associated with the downwash from intermediate disk loadings are more severe than those experienced from rotors. Thus, it would appear prudent at this time to do away with the emotionalism that accompanies discussion of this subject in several quarters.

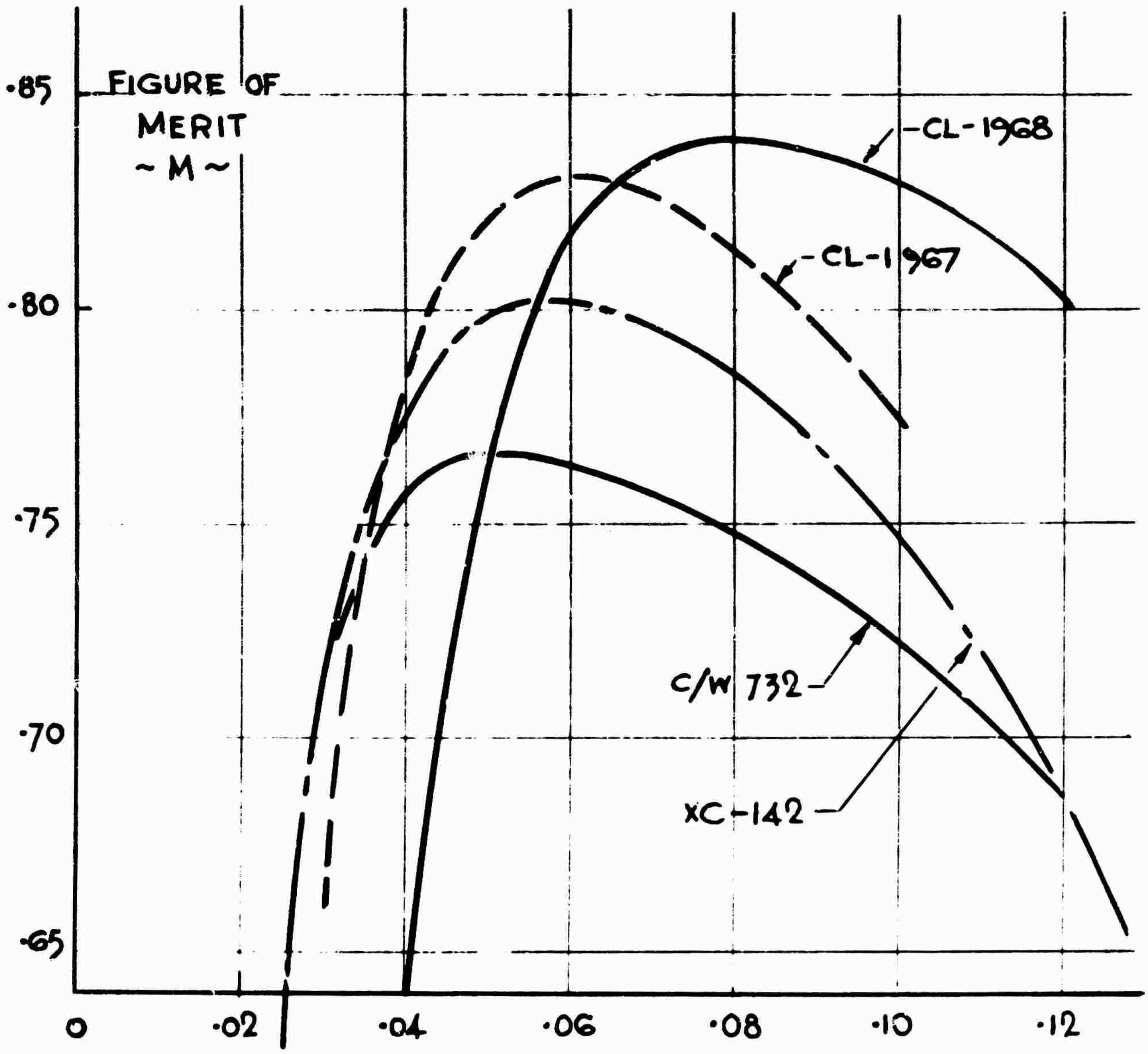
FIG. 1. PROPELLER STATIC PERFORMANCE.

A.F. = ACTIVITY FACTOR PER BLADE.

$$M = 0{\cdot}798\ \frac{C_T^{3/2}}{C_P}, \qquad C_T = \frac{\text{THRUST}}{\rho n^2 D^4},$$

$$C_P = \frac{550\ \text{SHP}}{\rho n^3 D^5},$$

n = REVS/SEC.
D = DIAMETER FT.
ρ = ATM. DENSITY SLUGS/CU.FT.
SHP = SHAFT HORSE POWER.



POWER COEFFICIENT ADJUSTED TO A.F. = 100
$C_P/(\text{A.F.}/100)$

FIGURE 2

EFFECT OF AIRCRAFT SCALING AT CONSTANT LOADING FOR

FIXED CONTROL POWER REQUIREMENTS

CL-84 Prototype Cyclic Configuration

Scale factor	1/2	1	2	3	4
Diameter - ft.	7	14	28	42	56
Hoverweight - pounds	2,500	10,000	40,000	90,000	160,000
Roll Control Power - rad/sec^2	1.75				
Pitch Control Power - rad/sec^2	1.00				
Roll Moment of Inertia - slug $ft^2 \times 10^{-4}$	0.125	2.0	32	162	512
Pitch Moment of Inertia - slug $ft^2 \times 10^{-4}$	0.094	1.5	24	121.5	384
Differential Thrust referred to 1/2 Hover weight	±17%	±34%	±68%	±102%	±136%
Cyclic Thrust Vector off-set referred to Propeller Radius	±11%	±22%	±44%	±66%	±88%

PANEL DISCUSSION

Question and Answer Period

CHAIRMAN: John E. Yeates
Chief, Aeromechanics Division
U.S. Army Aviation Materiel Laboratories
Fort Eustis, Virginia

PANEL MEMBERS:

Edward S. Carter, Jr.
Sikorsky Aircraft

Charles W. Ellis
Boeing Company - Vertol Division

Frederic B. Gustafson
NASA - Langley Research Center

J. M. Harrison
Lockheed-California Company

Robert R. Lynn
Bell Helicopter Company

O. E. Michaelsen
Canadair Limited

The question and answer period following the oral presentations by the panel members was tape recorded, and the text appears on the following pages. This material was taken directly from the tape and was not edited.

QUESTION AND ANSWER PERIOD

Henry R. Velkoff
Ohio State University

The question is to Mr. Ellis -- a very simple one. What airfoil section is on the advanced geometry blade? Is that the same one that you had indicated on the other slide?

Ellis:

It's a cambered section inboard about 18-percent thick, tapers to about 6-percent thick, with 6-percent thickness at the tip, with some camber in the tip section.

I haven't sorted out yet in my own mind how much of the damping is a function of the apparent damping, once we enter stall, as a function of the airfoil dynamic characteristics, and how much is a function of the structural characteristics of the blade -- we are in the process of doing that.

Velkoff:

When you speak of the airfoil characteristics, you are speaking of the boundary-layer separation, reattachment type of thing primarily?

Ellis:

Yes, there is some evidence from some of the fixed-wing art -- I've never seen it documented, but some of the fixed-wing art -- that you can control factors like the lift-hysteresis loop and presumably the moment-hysteresis loop on an airfoil by changing the sharpness of the leading edge. I don't know whether my data here is showing some of that effect -- the unsteady aerodynamic characteristics -- or whether it's, in fact, the structural damping of the blade that's important.

Raymond E. Rose
Honeywell, Inc.

Mr. Gustafson, you mentioned a self-adaptive trailing-edge section and you mentioned something to the effect that jet-flap concepts might be useful. Did you have in mind some sort of a jet-flap arrangement where you could change the camber effectively in a cyclic manner as the blade rotor goes around -- something of this nature?

Gustafson:

I would agree with that. It isn't actually what I had in mind. What I had in mind is that is we use the jet flap even without intending to have this effect, it's something that we need to take into account in assessing the moment characteristics of the blade and actually, I believe, that it will turn out to be a favorable effect. However, I would be happy to be given credit for what you just said also.

Jan M. Drees
Bell Helicopter Company

I'd like to ask Mark Kelly what the possibilities are for increasing the speed in the 40' x 80' Wind Tunnel?

Mark Kelly
NASA Ames Research Center

I haven't really thought too much about that yet, Jan, because I thought that what we really need is a new full-scale V/STOL wind tunnel that's properly designed from scratch to treat the testing of aerodynamic -- of the special V/STOL aerodynamic problems that we have. And so, I think we'd do a lot better if we just started from scratch and did the job right. There are some additional factors you have to worry about too. Those of you who have been out to the 40 x 80 know that it's not only the world's biggest wind tunnel but it's also the world's biggest pigeon roost, and in this day and age we have to worry about the social consequences of our actions. I don't know what we're going to do with all these pigeons when we put them out into the street when we rebuild it with these heavy-lift helicopters that Bob [Lynn] mentioned. We might also consider in both of these large facilities that the return from the pigeon guano might offset part of the cost. We need to take the big look at these things.

Richard P. White, Jr.
Rochester Applied Science Associates, Inc.

I'd like to direct a question to the eminent philosopher, Bob Lynn. It seems, Bob, that you've set a very ambitious time schedule for the solution of the noise problem, and I guess my question is this: Can we really accomplish what you think we can accomplish before we can initiate this into the original design procedure of a helicopter, and how can we do that until we fully understand the problems we are working with?

Lynn:

I think classically the experimentalist leads the analysis. I think Jack Rabbott pointed that out whenever he said, "I don't know why this parameter works, but here it is" -- in the aspect ratio correction on ground effect. Everything or most things that I have seen come from an understanding just as Gus was pointing out -- from an understanding of the flow you develop the means of handling it; you have to be able to visualize it first. So, although I was, don't misunderstand what I was doing, attempting to slant things toward what people should be working on and what might be the consequences, I think we can make major gains in noise before we get a complete analytical definition of it. I think the analytical definition of this may not be as far off as some people might think -- in fact, in talking to Loewy, I got that impression also. Does that answer it?

Waldemar O. Breuhaus
Cornell Aeronautical Laboratory, Inc.

This is a bit more of a comment than a question, but it's prompted by one of Mr. Ellis' remarks on the need for tailoring the handling qualities to the mission -- these very elusive handling qualities. Now, there is some work going on in V/STOL handling qualities requirements -- some work being done by Cornell, for the Air Force in this case -- in attempting to do some of this by classifying handling qualities requirements by mission phase, and there is discussion going on now attempting to do this by classification of aircraft which generally means size and performance. This is more the V/STOL aircraft than the rotorcraft work, though again there is discussion going on now about attempting to broaden this to include rotorcraft. The real difficulty that comes about in this, though, once you have gotten this matrix set up, is finding reliable data to fill in all the entries. This is the problem.

Ellis:

I was only suggesting that a proper use of moving base simulation techniques seems to offer a way to fill in the numerical values in the matrix. I am aware, in general, of the activities under way. As you say, the important thing is defining the numbers which should go in for any particular mission and any particular configuration.

Julian Wolkovitch
Mechanics Research, Inc.

Perhaps I ought to preface my comments with a statement that I run a small group labeled Guidance and Control. I want to make these comments from that viewpoint. I'd like to get remarks from the panel on this. I'm rather concerned by the development that seems to be taking place which you might call aerodynamics for the sake of aerodynamics. And I think it's very important that we don't get ourselves in the situation where it can be said that the aerodynamicist is somebody who assumes everything except the responsibility. We have responsibility to provide inputs to other areas such as structures, and guidance and control, and these areas do not need to know static thrust, for example, to one-percent accuracy. They need, for example, quick and dirty estimates of derivatives to plus or minus twenty-percent accuracy or even less accuracy than that. And, they'd like those estimates now rather than waiting for the wind-tunnel test to be completed in six-month's time or whenever the new wind tunnel is completed. Now, it's interesting, I think, to consider why it's so hard to get this data at least from the published literature. Now, I believe that in part this is due to a reluctance on the part of aerodynamicists to use simple, empirical analyses with fudge factors. Now, I believe -- and this is the particular point I'd like to get the panel's comments on -- I believe that fudge factors have become rather a bad word. I personally see nothing wrong with them provided they're clearly called out as such. In fact, I would very much like to see a format for a paper which begins with the title of the paper, follows with a list of symbols and then gives something headed "list of fudge factors". I think that it would be very useful -- it could be an extremely useful paper. Particularly to non-aerodynamicists who are often forced to use our work. That's my comment; perhaps the members of the panel would like to comment on that, with particular emphasis, perhaps, to the fact that many of the panel members are reviewers for journals and have a certain amount of responsibility in that area.

Lynn:

I think that if you didn't get the idea that I'm using fudge factors in what I expect to see come, and encourage people to do this until they develop a full understanding of it, I missed what I was trying to do. The empiricisms I was talking about -- I guess you could call them fudge factors here and there -- so I agree with you -- but remember also that empiricisms without the analytical backup are sort of barren. Nothing much will come out of them for the future. So it has to be a balanced approach.

Ellis:

I'd go one step further to say that not only are empiricisms barren but they're often apt to be dangerous. If you look at the history of most of our fudge factors, you find out that they arose because the actual performance deviated substantially from the guaranteed performance, and the chief engineer told the chief aerodynamicist, "Don't you ever do that to me again.". And so the aerodynamicist developed a fudge factor. And it's fine to use that fudge factor on the next design, provided you understand why it's there. If you don't understand why it's there, you're probably not going to be the chief aerodynamicist on the third design because you'll get fired when you miss the guarantee the next time.

Michaelsen:

I think I can pull out with Mr. Ellis, and I would like to point out that one of the purposes we came here for is to talk about research, not about providing data for other people -- I think we're incapable of doing that to some extent as one of the things we do suffer from is lack of understanding of some of these problems, and this brings me right back into what Mr. Ellis was saying -- that when you don't really understand what is going on it's very dangerous to dream up fudge factors.

Lynn:

Could I make one more comment? I would like to remind you that the lift, drag coefficient synthesized data sound pretty good. You know what they are? They're fudged data; that's what we all use. It's completely inaccurate, it's wrong. We don't understand quite why, but it's great if we're using it for small extensions from where we've been. Now we must accept that, and this gentleman's idea of the fudge factor -- I don't like the word because it sort of bothers me, and I'm sure that a lot of people wouldn't like it -- but we must accept the fact that we are using empiricisms and so it will always be, as long as we are extending the forefront of knowledge.

Carter:

I would like to make a brief comment in defense of the fudge factor. I think that the message I was trying to make there is that until we have a much more completely integrated handle on all of these elements that go into our problems, we've got to put these fudge factors in and, until we have the real confidence in the more particular details of these complete analytical methods, we're going to be forced to use these semi-empirical methods, at least as a check. No chief engineer is going to let a design decision rest on a stack of computer paper like this if it flies in the face of his own back-of-the-envelope empirical extrapolation, and this is the nature of our problem. Gentlemen, this is where we stand and, until we can come to a better complete package, we're stuck with it. One thing that struck me about not only this session but virtually all of the recent sessions that we've had is that when you compare our technical sessions in this area with sessions in other technologies, we very seldom see a paper on the application of these advanced methods that we hear about in the R&D programs -- the application of these efforts to one of our major weapons systems developments. The question I have to ask is, "Where are these advanced methods and these flow visualizations and these more sophisticated models when the chips are down and we have a program going with millions of bucks hanging on them?" Now part of the problem here, I think, is a responsibility that we all have to face up to. I think that when we've planned our programs, we've allowed ourselves to be pushed into schedules and financial commitments that don't allow a kind of moment-of-truth application of our more advanced methods to our going programs. Maybe some of this stems from the fact that, on the customer's side, there's perhaps more separation than would be optimum between the R&D people who are supporting the research that we see and the development program management people who are footing the bill on the going programs. But I don't think we're ever going to find out what these methods can really do for us, and get away from the fudge factor, until we are in the situation where the major going programs demand that we model these things before we design. We hear a lot about fly before we buy, but until we model before we release the drawings -- and until this is an understood part of the design cycle -- I don't think we're going to be forced to pull these methods together, and I don't think our chief engineers are going to be in the position to discipline this. I think that Bob's talk today really kind of put the carrot out in front of all of us. It was a very fascinating exposure. I heard a sort of stage whisper next to me about "We didn't realize the drug problem was that severe in Texas these days". But I think Bob stimulated a lot -- you can give a lot of thought to this. There is obviously a point of inflection in our rate of progress, looking back over the past thirty years and looking ahead ten years. We're just not going to get that by doing business as we've been doing it before, and we're not going to get it out of side R&D programs that aren't more closely related to our major weapons systems programs.

Harrison:

I think I detect that the speaker was asking for a more down-to-earth comment. He had a requirement for derivatives for small perturbation methods, and he wanted them immediately and had some difficulty in finding, say, a comprehensive list of analytical expressions for these in the existing literature. Commenting on the three types of math models I presented, what we would normally do is to start off using a perturbation model, say, for control synthesis, using the standard analytical expressions which go back to Lock-Bailey theory. We would follow that up as rapidly as possible, using the fully representative analytical model to generate the equivalent derivatives numerically, using the fully available nonlinear technique although these, of course, still involve fudge factors because, as it was pointed out, they were using quasi 2D airfoil data inside even the most sophisticated nonlinear, analytical model we've got yet. But it is possible for the customer to have, almost immediately, a set of numerically generated derivatives if he wants them for a small perturbation model.

Lynn:

In defense of the drug problem in Texas, can I say something? I was going to say, so that I hope everybody got the point, the other night I counted 242 specific, implied or stated research items in the written version of what I talked about. This is what I was trying to do -- give you a list without giving you a list. So to the first person who would send me a list of more than 242 things -- I'll send you five bucks.

Clifford D. Wells
Sikorsky Aircraft

I think there's uniform agreement among the panel that there is a real need for more airfoil testing, and I know we're doing a lot of work at Sikorsky, and Bell's doing work, and I think we also all agree that if we are given the tools we will be able to come up with airfoils that will fly in the transonic region with the fifty percent extension that Bob Lynn was talking about. But we have one serious problem in that there are really very few and maybe there aren't any suitable two-dimensional or three-dimensional airfoil test facilities available. There are several commercial facilities -- we have one in United Aircraft -- but the commercial cost is becoming so high that it's almost prohibitive. If you want to test in a commercial tunnel now, you start off at five- or six-hundred dollars an hour, and you can't do much testing within a reasonable budget at that price. So, I would like to ask Mr. Gustafson, is NASA or anybody else, do you have plans to come up with a suitable airfoil facility in the Mach range up through transonic and with the Reynolds number capability and the chords that we need, and also, at a price that we can afford, and that's the most important thing?

Gustafson:

I have to offer some reservations, but I would like to say something on the plus side if I could risk that without making things sound too rosy. There really is nothing I think that is what you want, that is, something where at any time, by paying a modest sum of money, you can simply walk in there with the right kind of thing and make the tests. On the other hand, the picture is nothing like I had begun to warn people that it was going to be and stay a few years ago. And I'm wondering what else I ought to warn people about so that I can see this wonderful occurrence that takes place when I try to get realistic. I thought that two-dimensional effort at Langley was gone forever, and I reluctantly began to so state and wa eople that capability would have to be developed elsewhere. It seemed I ha ardly finished saying that when I found out that in spite of these things that I had been told about how difficult it was, and some of the inside history on this sort of thing, and so on, that one of the primary facilities -- which had for a time been used for something else and then been allowed, I guess, literally to rust -- was being put back in shape to be a two-dimensional tunnel again. And I have since found that, in addition, there are two other supplementary, basically two-dimensional facilities to do that type of job, one of which is intended to be the sort of thing where somebody can get in it in a hurry. Now, it doesn't do really what you're asking for, but it should do what some other people would be asking for, and in a devious way be of some help to you. So there are some things that are on the positive side here. There really are three facilities. I think the thing that has sparked as much as is taking place, is really the work which, in our case, was started off by Whitcomb, in his own way and for his own reasons, on the supercritical airfoils which put new life into the whole subject. It's been kind of a dead-end street; as you probably all know you improve an airfoil and you take it out and run it in the practical condition and it acts just like the others. And some people got pretty allergic to that sort of thing. So it really died out for awhile, but this supercritical aspect, as it's called, more in our particular place, puts a different slant on the thing. It may affect, for example, other aspects. In fixed-wing aviation, it may have a sizable effect on the crossover point at which the more conventional type of subsonic-transonic jet airplane ceases to be the thing to use and the supersonic airplane comes in -- it may have a large affect on just that sort of thing. So the matter of airfoil development could not very well be neglected as that happened and, of course, there was an effort around to do something on the subject of aeronautics again, and so on. But I don't know of any hope to give you on being able to take just the right combination and walk in and get it. I think that, all told, we will have the capability to do that sort of thing, on a limited basis, which will probably primarily be for more fundamental purposes. Then, for other explorations, there will be less perfect setups -- I think that the blowdown situation provides one of them -- which can fill the gap in part. The part of the view being taken with the airfoil effort is that primarily we are going ----even though there is talk to my amazement of a series of airfoils. I never thought I would hear of it again. In spite of that, really the basic idea is to show the way, to show the sort of thing that works, and anybody who comes down with a confidential clearance can get a bit of a discussion on how it looks right now, for that matter. The

idea is to show the way toward this different approach to airfoil design and then leave it to others to use those principles and design their own. And, as far as I can see in general, they will have to test them somewhere else. But it may be interesting, while I'm holding the microphone, to mention a couple of things about this airfoil development. One is, that as many of you know, there has been, it has included a specific effort in the direction of airfoils suitable for helicopters and, to my horror, with complete symmetry -- no moment coefficient at all -- it's a nice place to restart. There has been a specific effort along that line -- not all of you know it -- and, also, the effort basically has reached a stage where we're finally getting something under way toward the flight article which will, of course, aim at the fixed-wing airplane -- it will be a fixed-wing airplane. It will be built for the purpose of putting this class of airfoil into actual flight situations with control surfaces on it, humidity in the atmosphere, whatever in the heck may make the difference and, actually, one reason you don't hear more about it is that having been stung before, we would like to try these things in actual practice and find out whether we should tell people that they really are going to work or not. I thought the general situation might be of interest to you, although I can't give you the answer you like.

Michaelsen:

I think that in regard to facilities, that there is a development up in Ottawa that may be of interest. The NAE have a 5-by-5-foot trisonic tunnel which is 20 atmospheres and they've just installed at Boeing's request -- actually Boeing, Seattle -- maybe people from Boeing here know more about this than I do -- have installed two-dimensional liners and are testing sections for Boeing at some reasonable price. They have a capability there of getting up to 40 million Reynolds number and, of course, with variable Mach number from 0.5 and up and also independent variation of Reynolds number and Mach number. This is something Boeing has latched onto at the NAE and they intend, to my knowledge, to test a minimum of 300 hours per year for the next five years, so I'm sure that the tunnel, having much more utilization than that, may be a facility that you can get into at a reasonable price and I think it's pretty unique in its capability at the moment.

John M. Duhon
Bell Helicopter Company

I'd like to make one more comment on fudge factors. I think it depends on what you're doing. If you're working in a complete research atmosphere, I think you should just steer clear of them but if somebody asks you for an answer one hour from now or next week, you have to resort to fudge factors to supply the answers for the quick work. I also would like to note that a lot of people are working on the hovering rotor or the static propeller and a lot of people are doing different things. Some people are doing flight visualization,

some boundary layer work, others might be measuring pressures. The thing that's important is that we need to measure all these things simultaneously on one rotor even if it's only one test point, so that the analytical model can be -- have something to have a target for. Right now, we don't have a target. So we're forced into matching thrust and power data but without an accurate detailing of the angle-of-attack distribution or the spanwise flow effects or what have you. So what we need is everything at once -- which is a very monumental task -- but, until we do this, we're going to be forced to fake it, just using empiricisms. I don't think it's a question, I think it's a comment which you may have some comments on.

Michaelsen:

I suspect that I just talk a little bit too fast, but the point that I was trying to make was that we are indeed already using a good deal of these techniques at the same time -- flow visualization, flow measurements, followed by determination of angle-of-attack distribution and strip analysis. We are presently doing surface pressure measurements. We have not yet contemplated doing boundary-layer measurements but we sure would like to, and I think the point made is very good, that it is a necessity for doing all these things at the same time on the same article in order to get the fundamental understanding of what's going on.

Yeates:

I think maybe at this time we might let the panel have time for about two or three questions for the panel themselves. Do any of the panel members want to ask another panel member a question?

Ellis:

I have one for Gus. Have you explored the question of exploiting a reflex trailing edge to control the pitching moments while, at the same time, gaining some significant advantages in $C_{L_{max}}$, for example? The evidence on the sections with smaller camber like the 23 series is that you can in fact do this -- you can set your pitching moments equal to zero and pay essentially no penalty for it in drag or $C_{L_{max}}$.

Gustafson:

Well, I'm surprised that you find that you pay no penalty for it and I hope you're right, but I really have felt the other way around. Back in the 30's,

there were torsionally flexible blades along with extremely high moment coefficients -- as we would see it with airfoils that have been used since -- flat-bottomed thick airfoils, Göttingen 606, Clark Y, whatever. Ten times the pitching moment that we think of is something to worry about subsequently. And, of course, this produced a problem that I didn't mention -- in effect, this feathered the blade in such a way that the stick came back against the aft stop and the aircraft wanted to do an outside loop. I think people down my way sort of over-reacted to that, as I see it. They then recommended the 230 series reflex so that there would be no moment coefficient at all. And it has been my impression that this was an unfortunate thing to do on two counts. It's been my impression that the reflex killed most of the gain that we would otherwise have had and that it was not necessary to -- perhaps not even desirable -- to run this all the way to zero moment. There may be something in this if we come at it in a different way, but my own line of thinking is away from what you are saying, partly for the reasons that I've given and also, because in some of these newer airfoil designs, I think we're going to want to put the camber in at the back. So I don't say there's nothing there, but those are my comments.

Ellis:

In regard to the reflex 23 series, we're flying that on the 47 series now and we see about a -- almost a 40-percent gain in the thrust capability at a given limit rotor speed.

Gustafson:

Well, I think you ought to get some gain and I think, when people couldn't find a gain, that one of the problems was that they were not looking at it in the light that I think you are now -- in other words, they were failing to look at the effect on the limits of operation. They were trying to measure perhaps with manifold pressure an effect on power at a modest speed, and this is one of the reasons that things stayed submerged for so many years. I am surprised at the amount of the gain. It does seem as if the things that we can do from logic, or two-dimensionally, generally go in the right direction but sometimes by some rather surprising amounts in one direction or the other; and I did have the impression that there were simultaneous changes in getting that 40 percent. Is this really from that one change?

Ellis:

Essentially from the one change.

Gustafson:

Well, it's a little unusual to have only one change, if you don't mind the skepticism. I'm for camber -- even in the front end, but I'm thinking farther aft myself.

Lynn:

I have a question for Chuck. The downwash effect that you talked about was whenever the rotor is horizontal and you have sort of a sterile laboratory condition and everything is flat; the aircraft is not flaring. It's been my impression ever since NASA first did that, that somehow we must introduce flare techniques -- I mean, when you flare a rotor, then the direct downwash underneath the rotor becomes important. That's when you blow manhole covers around and that's when you turn over cars. So, I was wondering if it really isn't disk loading and gross weight that we must consider as opposed to just weight or just disk loading and, also, I sort of have the feeling that operational people will learn to use or accept what they have to, within some reason perhaps. In other words, I think they'll be able to take higher disk loadings, as they must, to get more efficient aircraft. Now you're the one who brought up the disk loading question. How do you think of that -- and don't put emotionalism into it?

Ellis:

I'll add one other factor, and I'm not trying to make any case there; I brought that up merely to point out the fact that I don't believe we have a unified approach to the problem of what is a satisfactory, what is the real operational environment on the ground that satisfies all the facts that we have in front of us. I suspect that, in addition to disk loading, which is certainly important in some parts of the flight profile, and in addition to gross weight, that span loading is probably very important. I've seen a UH-1 and a UH-2 hover side by side in the sand at Yuma. They both have the same rotor diameter, and they both have the same gross weight. One disappeared in the sand cloud, and the other one was completely visible. The only difference there is two blades versus four blades, and I can only say, well, that must be a function of span loading. I'm sure that there are at least three important variables here and I think that, if we're going to get bigger aircraft with higher disk loadings, higher span loadings, that we should spend some effort to understand what it is that picks up the manhole cover. Is it the fluctuating pressure from the vortex hitting the ground, is it the average pressure from disk loading, or is it the integrated forces from the wake? I don't know the answer to this.

Carter:

This isn't a very critical one, I think, but, Chuck, I just want to follow up on Hank Velkoff's question to be sure we understood the comparison that you showed us on the control loads. Did I understand that there was a difference in the airfoil section as well as in the material and as well as in the structural stiffness -- both flatwise and torsion -- in those two rotors?

Ellis:

There is a difference in the airfoil section, and there is a difference in the material. There's no significant difference in the stiffness. The frequency ratios are similar, including torsion. But the airfoil sections are different and the material is different. Now, from wind-tunnel tests -- I didn't mention this during the discussion -- but from wind-tunnel tests of a CH-47C rotor and that particular rotor system, made both from the same material with the same techniques, you don't see the difference in the growth rate of the pitch link loads. You do see a difference -- which I attribute to the difference in airfoil characteristics -- on the point of initiation of stall, but you don't see a difference in the growth rate. So, I tend to attribute the difference in growth rate to the material.

SOME PROBLEMS IN THE NUMERICAL SOLUTION OF VORTEX MOTION EQUATIONS*

J. P. Jones and M. Noak
UNIVERSITY OF SOUTHAMPTON, ENGLAND

**Messrs. Jones and Noak were scheduled to present a paper during Session I (Volume I of these Proceedings). Unfortunately, neither gentleman was able to attend the Symposium. The following paper is being published here because of the general interest of the subject matter even though the title differs from the originally scheduled presentation.*

SOME PROBLEMS IN THE NUMERICAL SOLUTION OF VORTEX MOTION EQUATIONS

by

J.P. JONES and M. NOAK

Department of Aeronautics and Astronautics
University of Southampton
United Kingdom

SUMMARY

The results of some flow visualisation experiments on the trailing vortices from a model helicopter rotor are presented. It was found that the wake contraction was a cyclic phenomenon dependent on the number of blades. Its precise cause could not be ascertained but was probably due to interference between the blade bound vortices and the trailing vortices in the rotor wake.

An investigation was conducted into some of the problems encountered when attempting to solve numerically the equations governing vortex motion. The accuracies of the fourth order Runge Kutta method and the Euler method were compared at the theoretical solution when solving the equations of motion of two infinitely long line vortices. These two numerical integrations methods were then used in computing the motion of two ring vortices in close proximity.

From this work it was clear that the Euler method gave results which, when compared with the theoretical solution, showed a growing error with an increasing number of iterations. The fourth order Runge Kutta produced results which were in complete agreement with the theoretical results.

SOME PROBLEMS IN THE NUMERICAL SOLUTION

OF VORTEX MOTION EQUATIONS

INTRODUCTION

This paper is partially a progress report on research which has been conducted at Southampton and a report on some detailed investigation performed. It falls in two parts, a short section on helicopter rotor flow visualisation and a larger one on an investigation into numerical integration problems arising from calculations on vortex motion.

A study was made of the paths followed by the vortices trailed from the blade tips of a hovering rotor. This was deliberately run with a low tip speed and inflow so that it was possible to use continuous smoke emission for flow visualisation. The flow patterns so produced were then photographed. During the course of the tests, the number of blades was altered to change the rotor solidity. The positions of the vortex cores were measured and presented as a graph of radial position against distance downstream of the rotor (see Fig.0).

A cursory examination of these graphs indicated that the wake contraction is not a smooth process. As has been found by others, the trailing vortex remains substantially in the plane of rotation until the close approach of the following blade. It was also noticed that, in the two

bladed rotor case, the vortex path clearly oscillated about a mean path (Fig.O(b)). This oscillation completed two cycles in approximately one rotor revolution. When examined closely, it was possible to detect a similar phenomena in all the other results.

A mean path through the experimental points was drawn on all the graphs. Perpendiculars to this mean curve were then dropped from the experimental points. The differences in the radial and axial positions between the experimental points and the intersection of their associated perpendicular with the mean curve were measured. These measurements were subjected to a fourier analysis based on the 'age' of the element expressed in degrees of rotor rotation since it was trailed. It was found that the coefficient whose argument corresponded to the number of blades multiplied by the rotor rotation was significantly larger (2 to 5 times) than the others.

The cause of the phenomenon is uncertain. It could be the result of interference between the trailing vortex system and the blade bound vortices, it could be due to bad tracking; most likely it is due to a combination of the two. This effect on the flow is not unknown, Crimi has already produced a mathematical model whcih gives a rather distorted flow behind a two bladed rotor in forward flight. (Fig.11).

It was decided to undertake an investigation into this phenomenon using a mathematical model. Unlike the models used so far, this model was to be capable of generating a wake without using any initial assumed wake shape. Various theses and papers were consulted and in particular one by Butter stood out. In this thesis, he had calculated the motion of the shed

vortex sheet behind a wing to find out how the tip vortices were formed. He had also used his method to calculate how two infinitely long line vortices of differing strengths would behave and had found that his results slowly diverged from the correct solution with increasing calculation time. As this divergence depended on the integration step size, he assumed that by reducing this, he could obtain an accurate solution.

A number of other authors have used this method of integration, the Euler method, for vortex motion calculations. Thus, partially to gain experience in this field, it was decided to investigate the accuracy of several methods of integration.

The purpose of this investigation was to find out what parameters might be of critical importance when attempting numerical solutions of the differential equations describing vortex motion.

THE NUMERICAL MODEL

At an earlier stage we had examined the motion of two infinitely long line vortices. Their circulation was in the same direction but the vortices had different strengths. (Fig.1). The equations governing their motion are:

$$\frac{d}{dt}(X) = -KY/(X^2 + Y^2) \qquad (1a)$$

$$\frac{d}{dt}(Y) = KX/(X^2 + Y^2) \qquad (1b)$$

In these equations K represents the sum of the circulation, t is the time and X, Y refer to the distance separating the vortices in the x and y directions respectively. The solution of these simultaneous differential

equations represents the circular motion which the vortices have about their centroid of vorticity. Thus we had an exact solution against which we could compare the results given by any numerical integration of these equations (1a) and (1b).

The Euler method of integration is very commonly used because of its simplicity. The fourth order Runge Kutta procedure is well known for its accuracy but is slow. These two methods were chosen and their results compared with the analytic answer.

When the computer program was set up, we found it oportune to express the integration step as the time required to achieve a specified angular rotation of the vortices about their centroid. The integration was then performed by the two methods for a range of step size between 10° and 45°. The results showed that the Euler method is unsatisfactory (Fig.2). Here we note that the vortices spiral outwards from their original positions. This rate at which they spiralled outwards depended on the step size chosen, rising as the step size increased. It can be shown conclusively that a simple harmonic motion is unstable when integrated by the Euler method.

However, the results produced by the fourth order Runge Kutta method were far more satisfactory. As Fig.3 shows, there is little tendency to spiral outwards even with larger step sizes. We found that there was no spiraling with step sizes of 45° or less. To see what would happen if a less accurate integration method was chosen, we used a second order Runge Kutta procedure. Fig.4 shows the results obtained from this method with step sizes of 10° and 45°. It is clear that the lower order method is less satisfactory

but it is quicker.

Having discovered that the Euler method may lead to significant error within a short time in a very simple vortex problem, we decided to investigate the errors which might occur in a more complex system. The problem of calculating the motion of two isolated ring vortices was chosen. In this problem, it is necessary to integrate in space as well as through time. Thus there are two phases where significant errors could occur according to the method of approach to this integration. This system has no exact analytic solution but is still simple enough to permit meaningful examination of the effects produced by the integration methods and the approximations used to describe the rings.

Since the problem is symmetrical about the z axis (see Fig.5), we only need to know the radial and axial motion of one point on a ring to know how that ring moves.

Writing down expressions for the velocity of a point on one of the rings (see Fig.5 for notation) we have

$$\vec{V_1}(t) = \frac{\Gamma_2}{4\pi} \frac{\{\vec{r_1}(t) - \vec{r_2}(t)\}}{\{\vec{r_1}(t) - \vec{r_2}(t)\}^3} \wedge \vec{dr} + \frac{\Gamma_1}{4\pi} \frac{\{\vec{r_1}(t) - \vec{r_{1_a}}(t)\}}{\{\vec{r_1}(t) - \vec{r_{1_a}}(t)\}^3} \wedge \vec{dr}$$

The second integral contains a singularity when $r_1(t)$ and $r_{1_a}(t)$ are coincident and it is necessary to make an approximation to avoid this. To make the vector multiplication possible we replaced the rings by polygons of side dr and applied the Biot-Savant Law to the triangles formed by the points under consideration. The unit vector resulting from the vector multiplication

then gave the $\vec{i}$, $\vec{j}$ and $\vec{k}$ components of velocity. This polygon approximation gave a possible way round the singularity. By considering the motion of a point defining a corner of the polygon, and ignoring any velocity contribution due to the two line vortices forming the corner, the second integral became finite.

At this stage, we split V(t) into its radial and axial components and by integrating these simultaneously, found how the rings moved. (Appendix 1 for detail). This integration was performed using the Euler and fourth order Runge Kutta methods. During the course of investigations, the number of sides of the polygon was varied between six and thirty-six.

Some of the results are presented as Figs.7 to 10. Comparing results obtained for 6 and 36 sided polygons using the fourth order Runge Kutta (Figs.6 and 8) we see that the radial and axial mutual velocity diagrams are substantially similar, the major difference being a general slight reduction in peak values and an increased width of the peak. The axial self induced velocities are different, a result of ignoring the contribution of a larger part of the ring in the hexagon. Fig.8 shows clearly that the radius variation is the same for both cases. As expected from the axial velocity change, the axial spacing on the space plot is changed.

When, however, we compare the results obtained by the Euler method with those obtained using the fourth order Runge Kutta for a 36 sided polygon, a rather different picture is obtained. From Figs.6, 7, 8 and 9 it is clear that the magnitudes of both maximum and minimum velocities in the Euler method declined with increasing time, and that the period taken for the rings to reverse positions in space has increased by 20% for the first cycle and

100% for the second. This increase shows clearly on the motion plot (Fig.9). Figs.7 and 9 show the velocity and the space plots for a hexagon using Euler's method. The magnitudes of the velocities differ substantially from those of the compatable fourth order Runge Kutta and the period has increased by 100%. The clearest indication of the unsatisfactory results that the Euler method produces is shown on Fig.9A. This clearly shows the drastic effect that changes in time and space integration step size have on the motion of the rings. When halving the maximum permitted change in radius or Z axis position, the space plot is considerably changed.

These calculations were also performed using twelve and twenty four sided polygons. We found that the Euler method was as unsatisfactory in these calculations as it was in the calculations for the two ring vortices shown here.

From these results we can only conclude that the Euler method is incapable of giving an accurate solution in a problem of this nature.

CONCLUSION

I would now like to pass some comments on some previous work. In 1935 Westwater produced an A.R.C. report on roll up behind an aerofoil. Fig.10 shows the form into which his calculations predicted the sheet would distort. We took note of the odd kinks in his curves and wondered what caused them. At the last Symposium P. Crimi gave a paper on the Prediction of Rotor Wake Flows. In this paper there is a diagram of the wake trailing behind a two bladed rotor in forward flight (Fig.11). This shows substantial wake distortion which he attributes partially to the perspective and the rest to the integration increment chosen. It is interesting to note that both these

authors used the Euler method for integrating in the time plane.

We have shown in this paper that this method of integration is unsatisfactory for relatively simple problems. From this evidence we ask whether the odd kinks in Westwater's work and the distorted flow patterns in Crimi's paper genuinely exist or whether they are due in part to errors accumulating in the integration.

Since the Euler method leads to a rapid accumulation of errors, it is most probably that the kinks etc. are due to these errors. It would be interesting to know by how much the accuracy of the calculations has been affected by these errors.

Until this is shown to be negligible, an unlikely possibility in view of our results, it must be assumed that the vortex paths shown by Westwater, Butter, Crimi and other reports where the Euler method of integration was used, are of doubtful veracity.

THE FUTURE

We hope to repeat Westwater's calculations in the near future using various methods of integration and to compare our results with his. At the same time we will continue to explore other integration techniques on the two ring vortex problem. One problem does arise here. Using the Euler method it takes about 120 seconds of computing time on a I.C.I. 1907 to complete 100 movements of the rings using a 36 sided polygon. When using a fourth order Runge Kutta, this takes typically 800 seconds. A part of our effort will thus lie in investigating methods of integration which, whilst lacking the precision of a high order Runge Kutta method, still give satisfactorily accurate results.

REFERENCES

1. WESTWATER, F.L. — Rolling up of the Surface of Discontinuity behind an Aerofoil of Finite Span

   A.R.C. R.& M. 1692

2. BUTTER, D.J. — The Downwash Field behind a Swept Wing at Low Speed

   M. Phil Thesis, Queen Mary's College, London

3. CRIMI, P. — Prediction of Rotor Wake Flows

   CAL/USAAVLABS Symposium Proceedings, June, 1966

4. CRIMI, P. and BRADY, W.G. — Presentation of Propeller Wakes by Systems of Finite Core Vortices

   CAL Report No. BB-1665-S-2

LIST OF SYMBOLS

Experimental Work

r	radial position
Z	axial position
R	rotor radius
R'	$\frac{r}{R}$
X'	$\frac{Z}{R}$
ψ	wake age in degrees of rotor rotation

Two Infinitely Long Line Vortex Models

Γ_1	circulation of 1st vortex
Γ_2	circulation of 2nd vortex
K	$(\Gamma_1 + \Gamma_2)/4\pi$
$x(1)$, $y(1)$	x, y co-ordinates respectively of 1st vortex
$x(2)$, $y(2)$	x, y co-ordinates respectively of 2nd vortex
X	$x(2) - x(1)$
Y	$y(2) - y(1)$
t	time

Two Ring Vortex Model

Γ_1	circulation of 1st ring
Γ_2	circulation of 2nd ring
R1	position vector for 1st ring

R2	position vector for 2nd ring
T	time
V_r	radial induced velocity component
V_{am}	axial mutually induced velocity component
V_{as}	axial self induced velocity component

Radius and Axial positions non dimensionalised by dividing these parameters by original radius.

INDEX TO FIGURES

FIG. 0

EXPERIMENTAL RESULTS

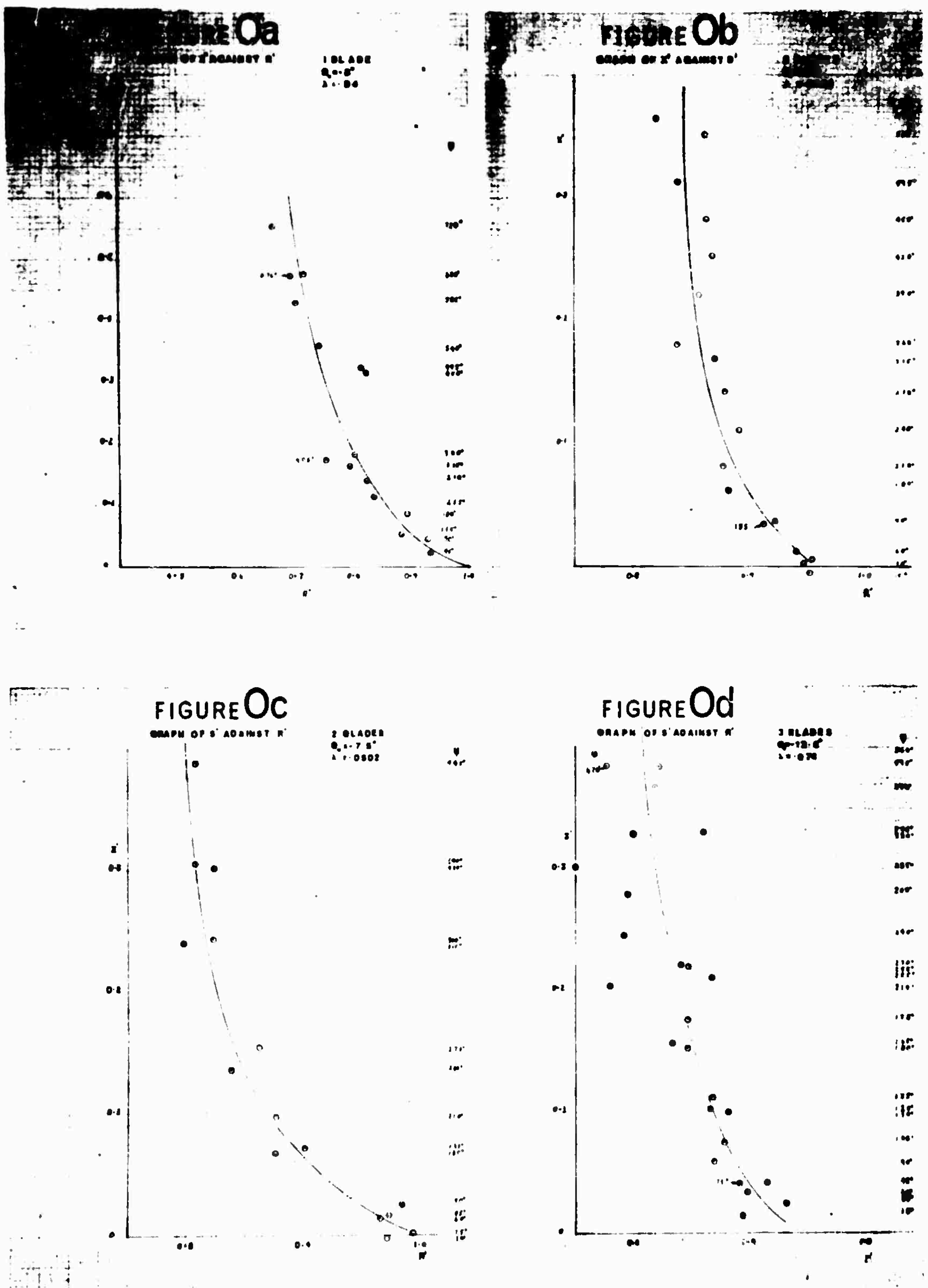


FIG. 1

TWO INFINITE LINE VORTEX MODEL

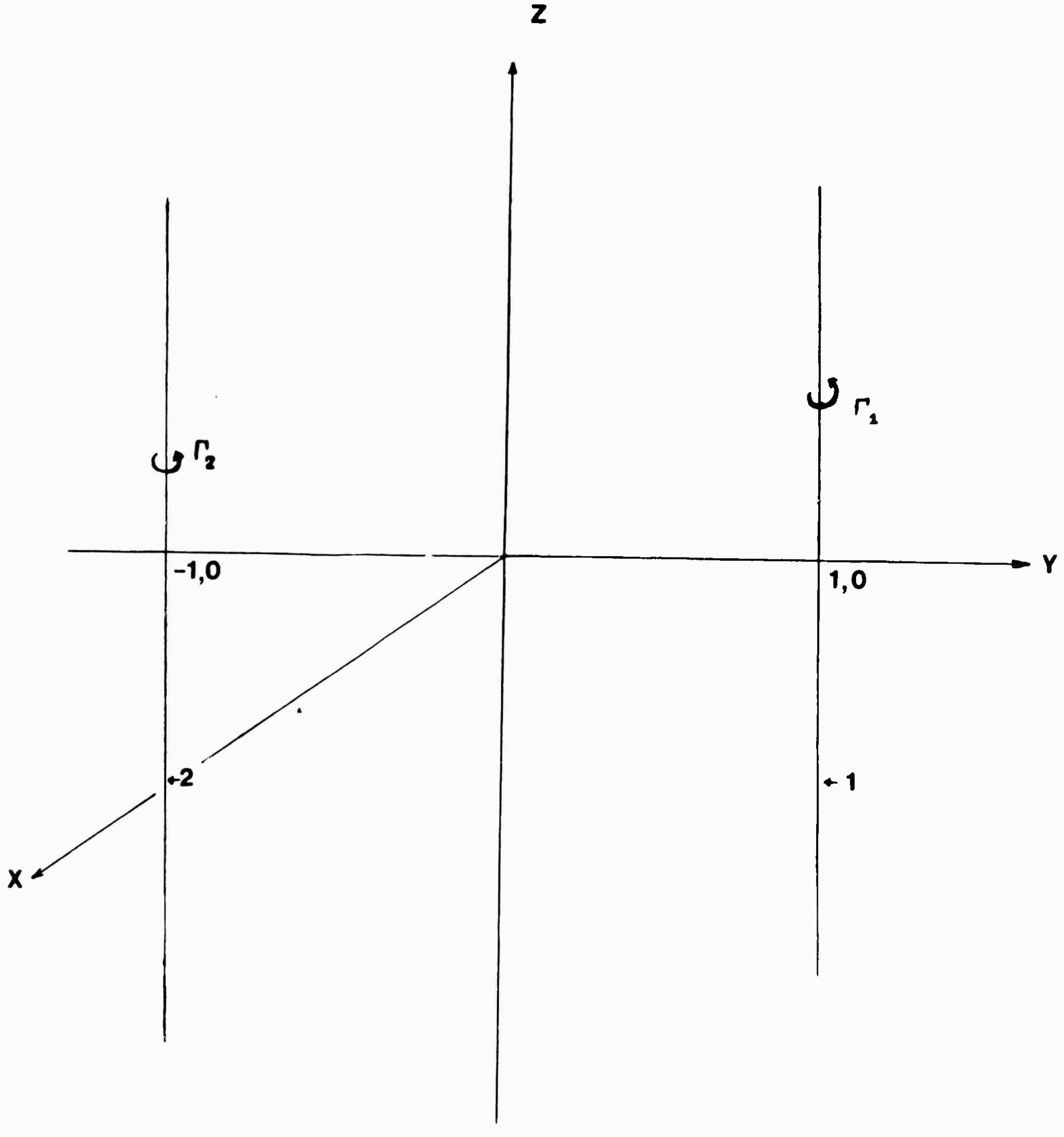
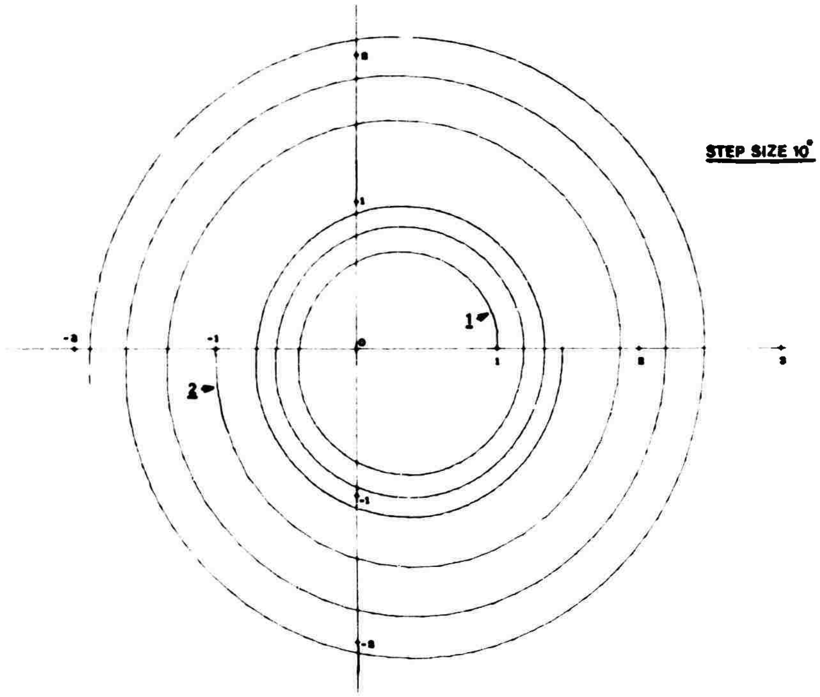


FIG. 2

EULER METHOD



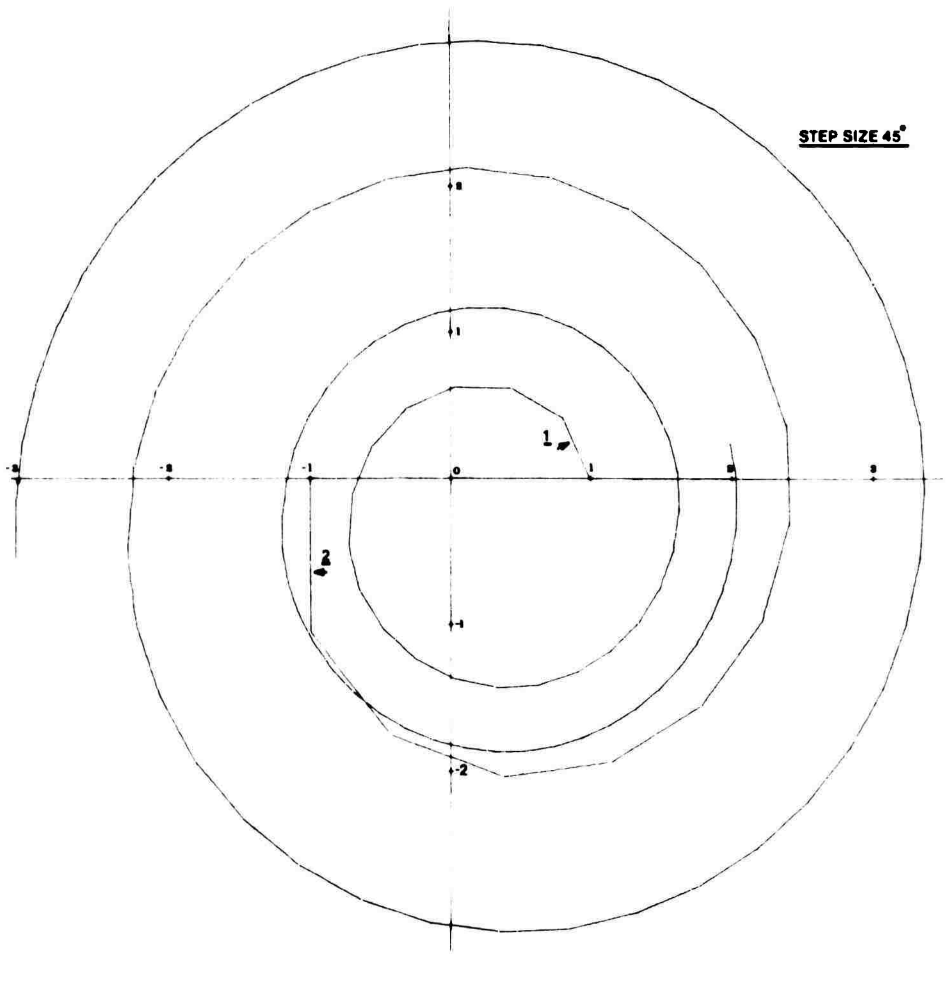
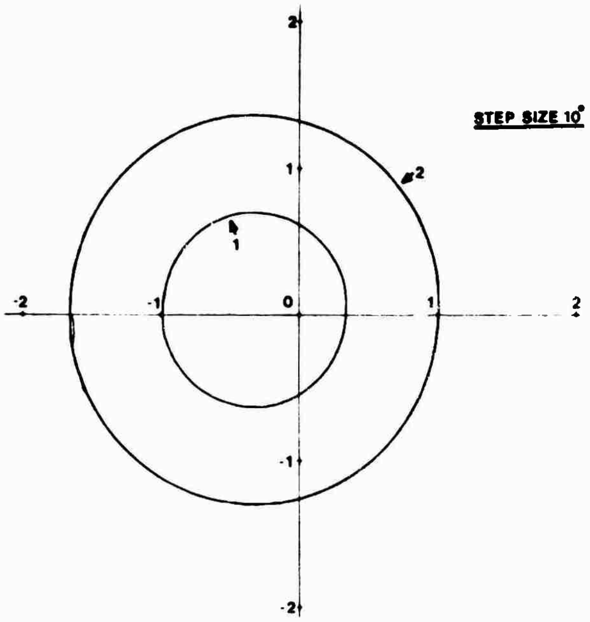


FIG. 3

4 th ORDER RUNGE KUTTA METHOD



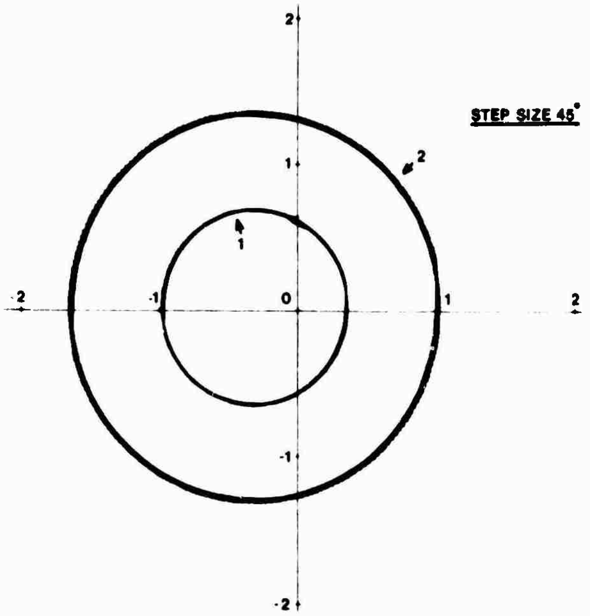
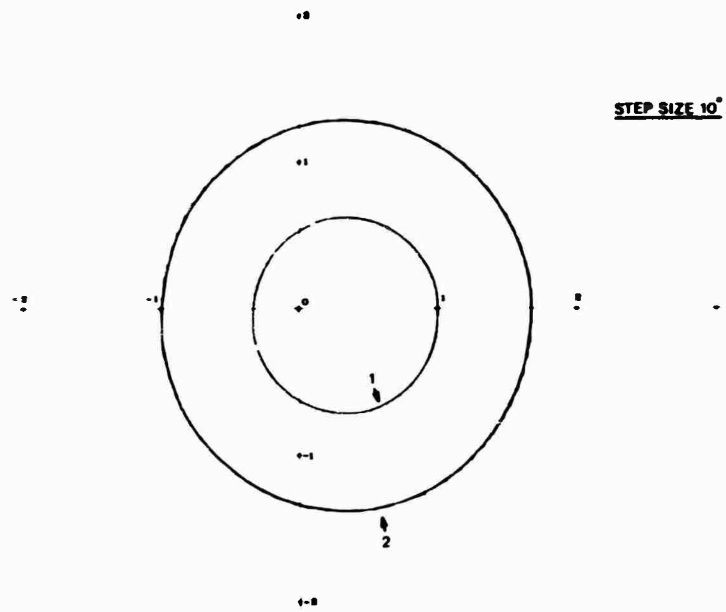


FIG. 4

2nd ORDER RUNGE KUTTA METHOD

STEP SIZE 10°



STEP SIZE 45°

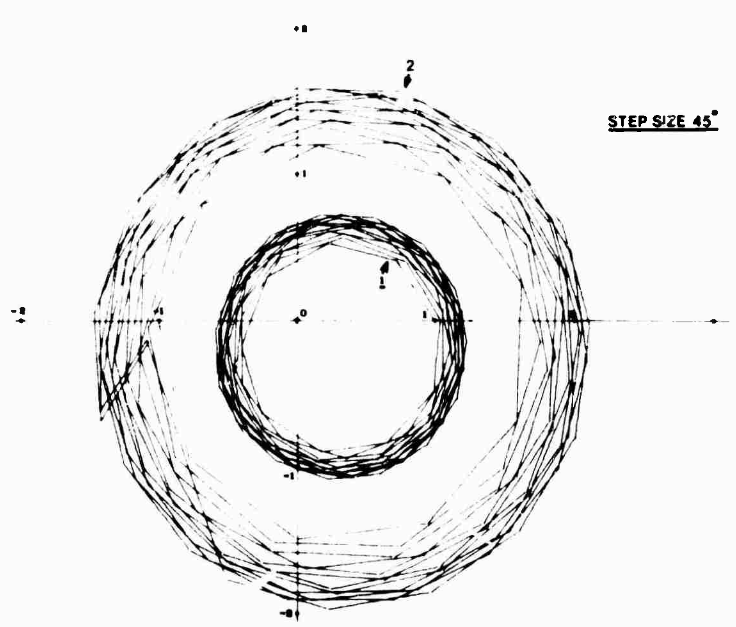


FIG 5

TWO RING VORTEX MODEL

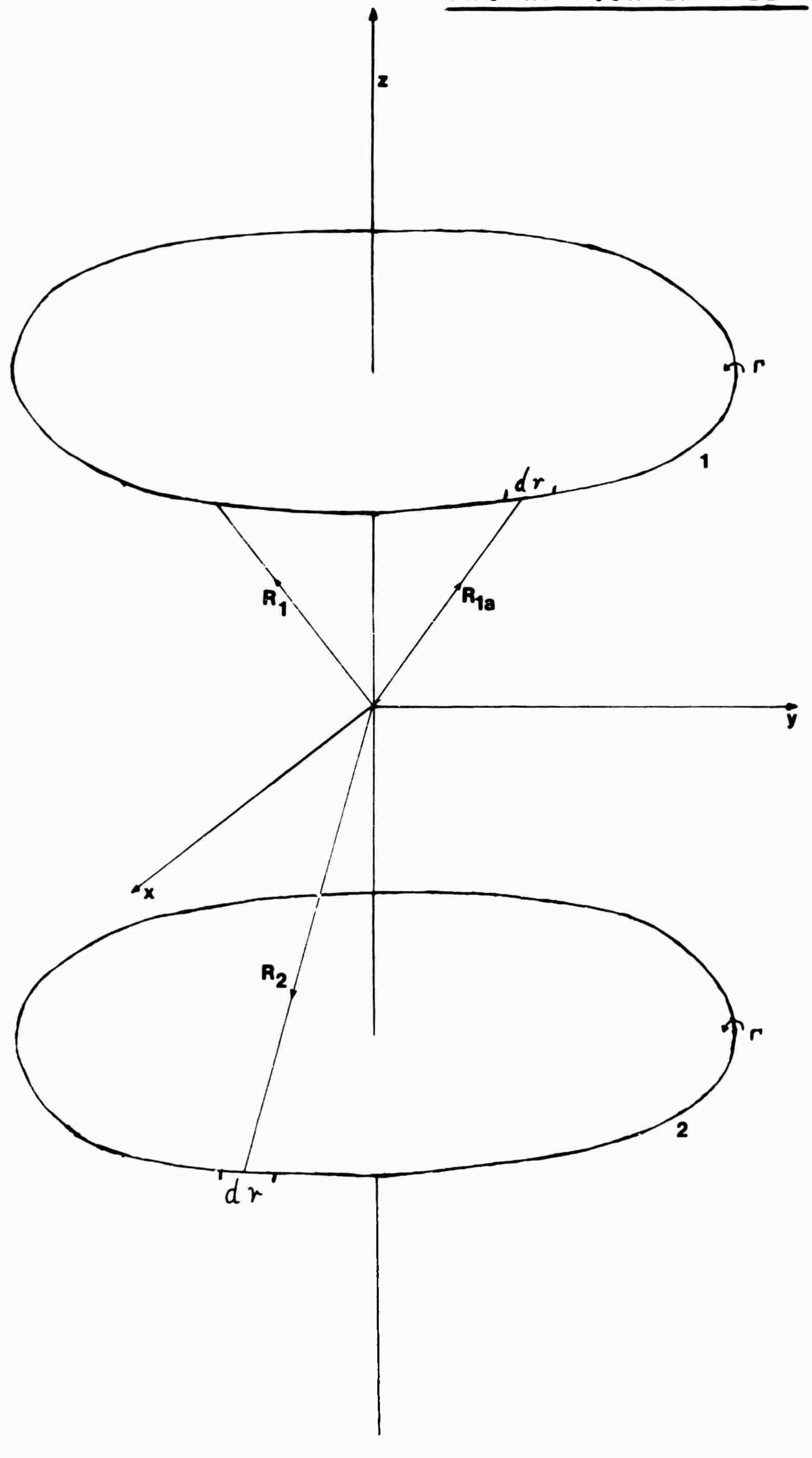


FIG. 6

4 th ORDER RUNGE KUTTA METHOD

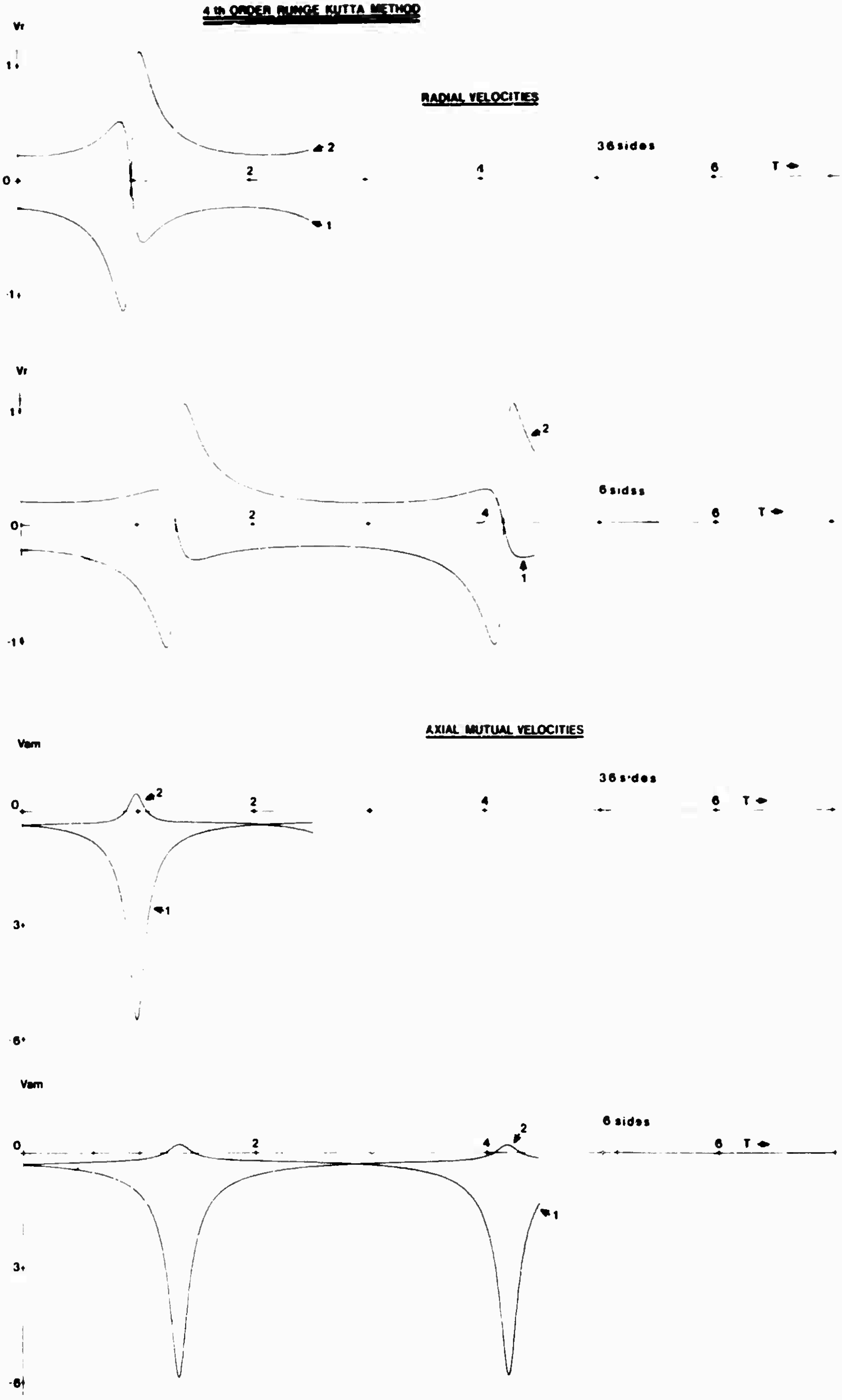


FIG. 7

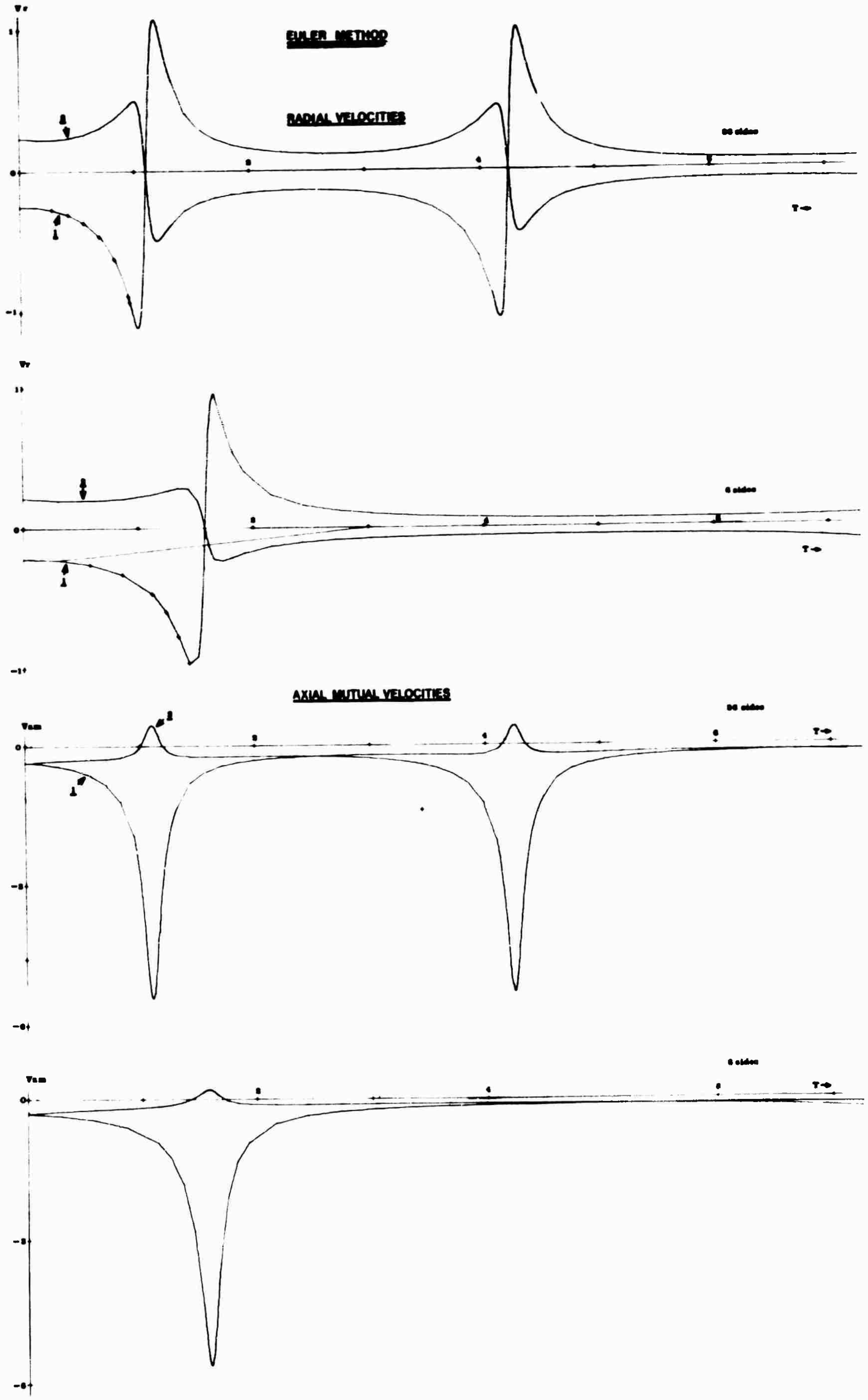


FIG. 8

4th ORDER RUNGE KUTTA METHOD

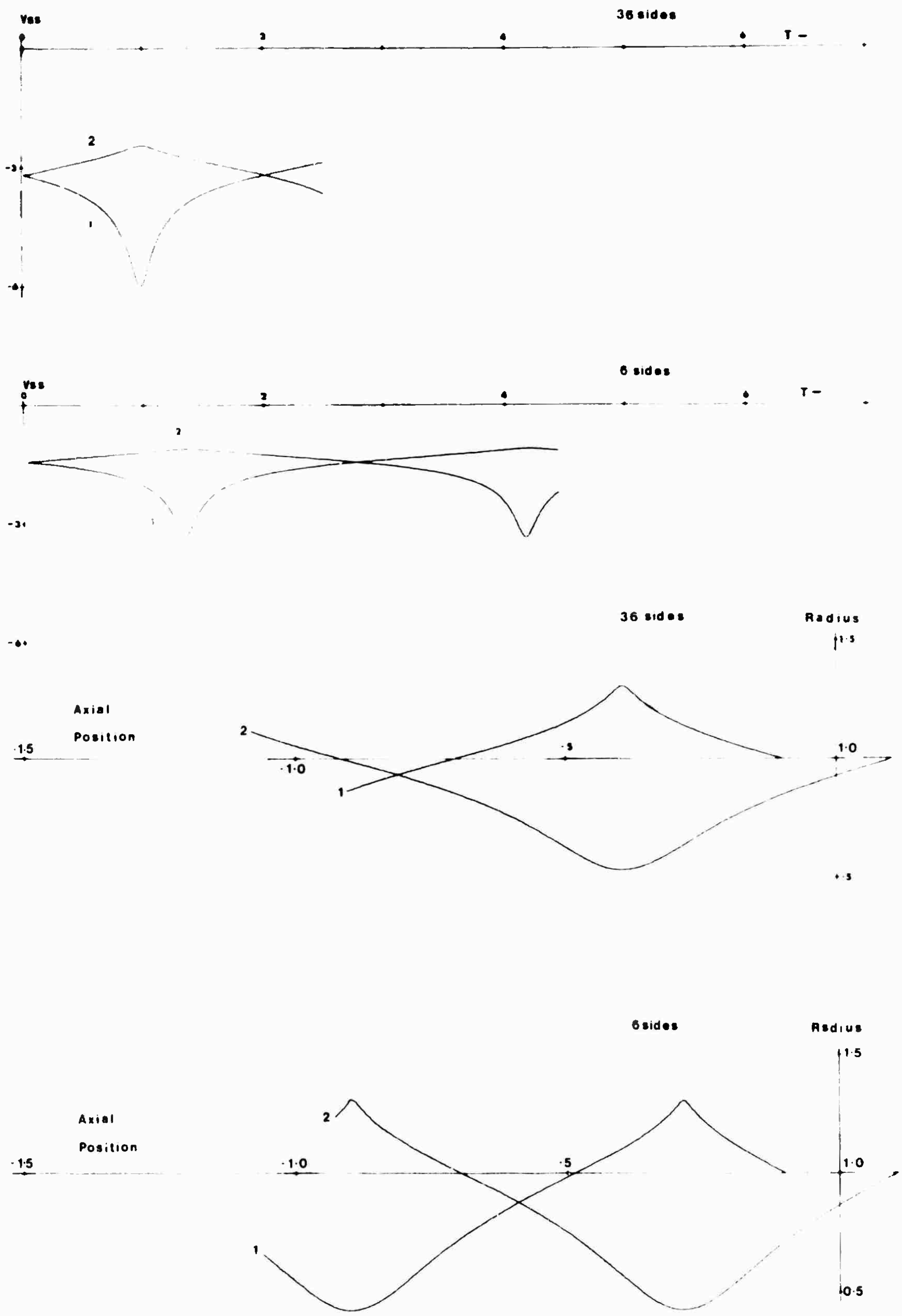
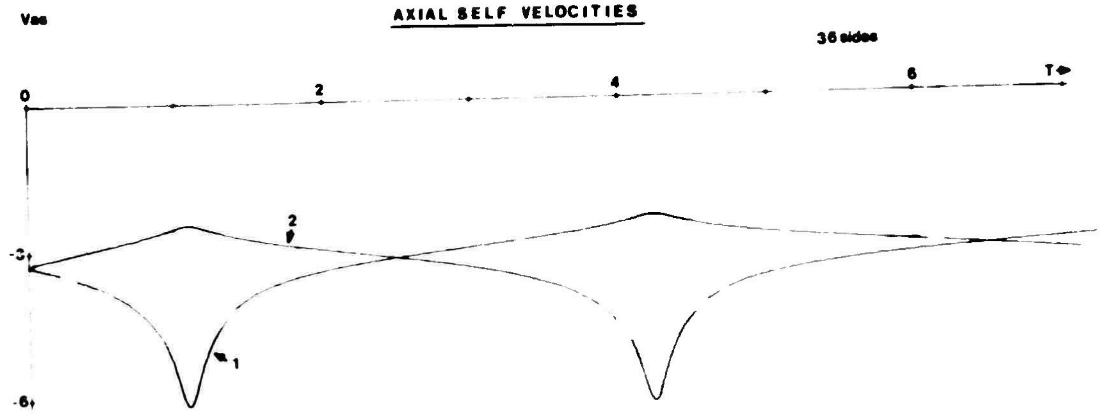
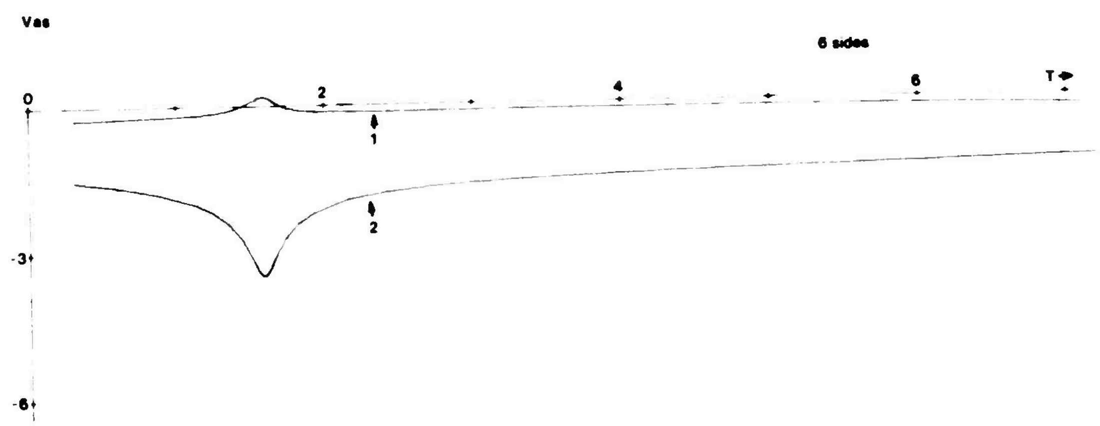


FIG. 9

EULER METHOD

AXIAL SELF VELOCITIES





SPACE PLOT

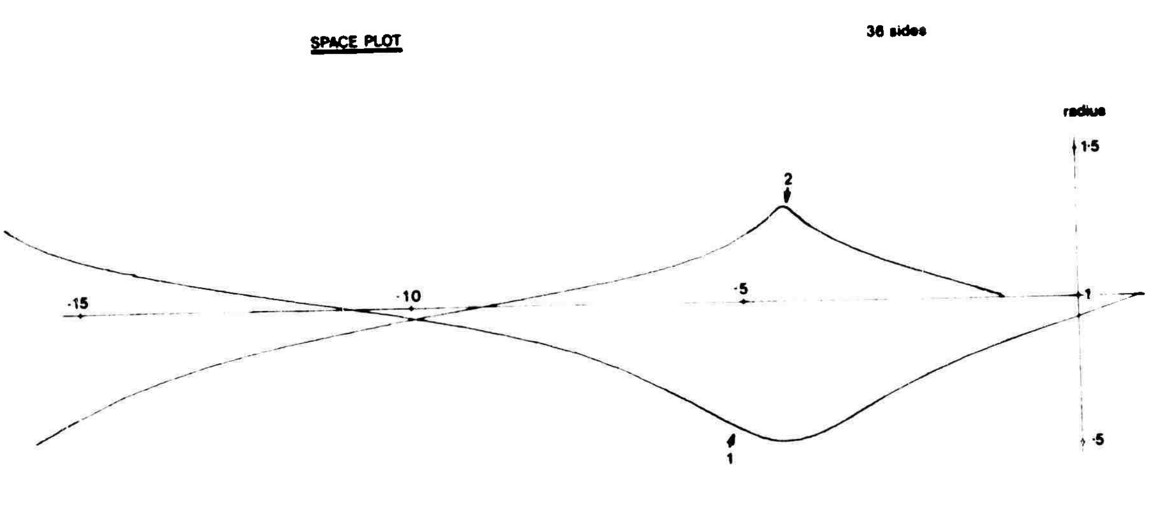


FIG. 10

ROLL-UP FOR WING ACCORDING TO WESTWATER

ITERATION

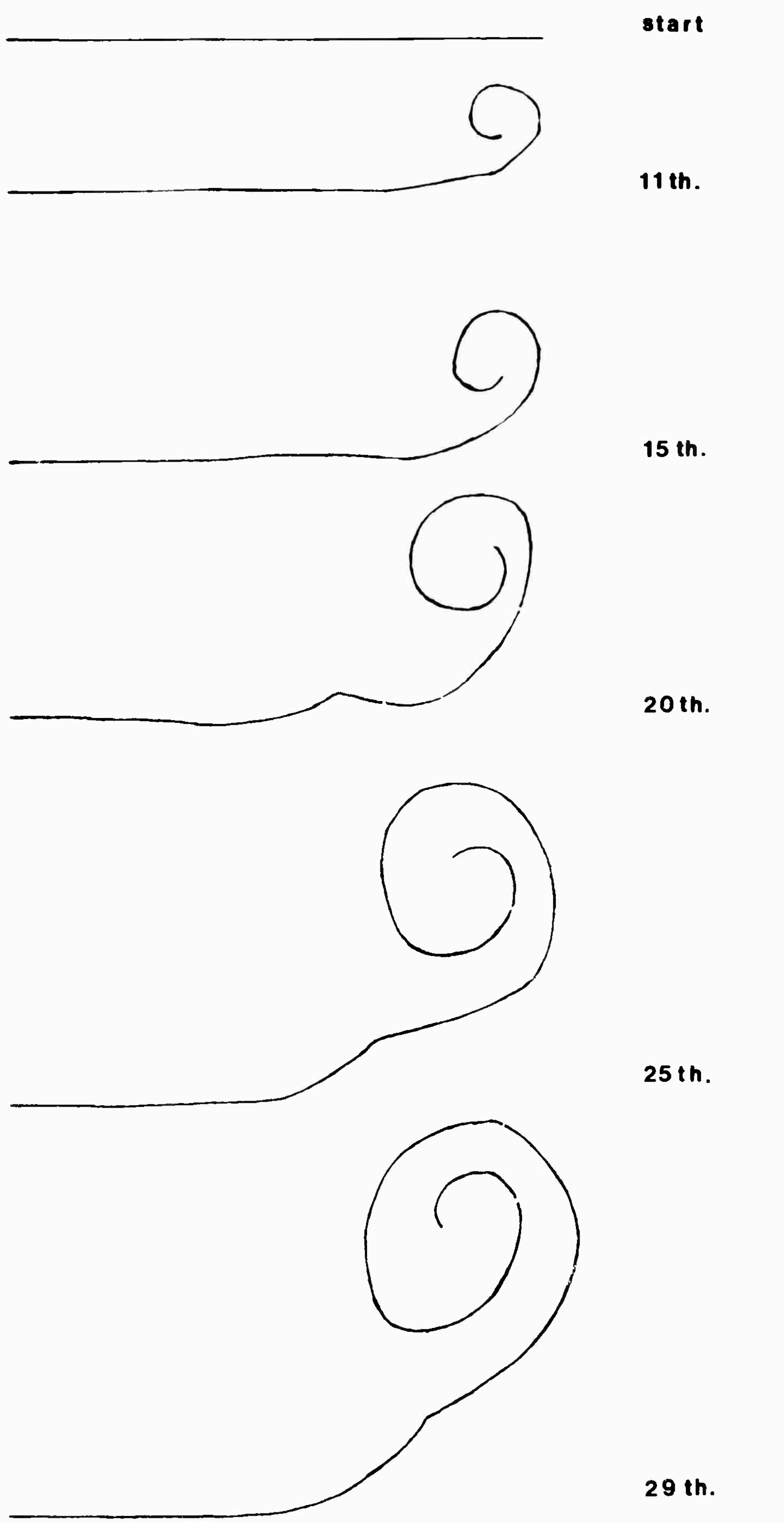
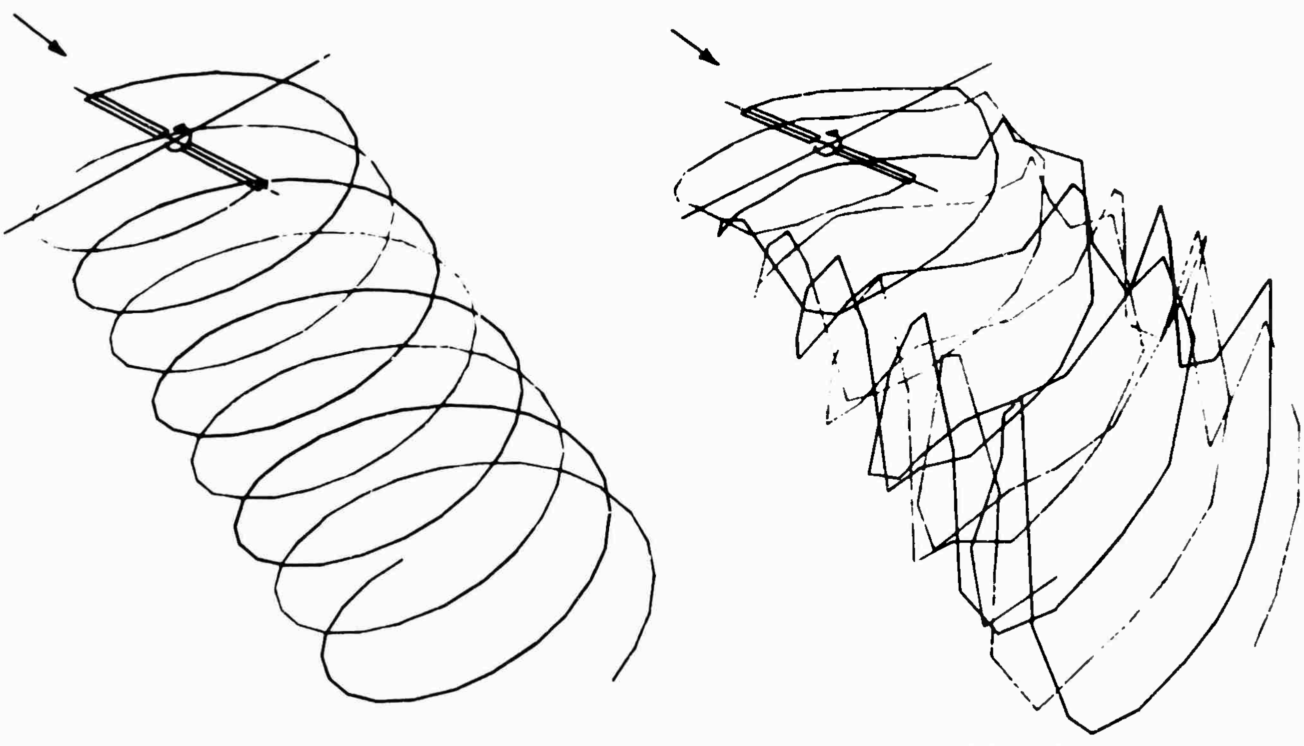


FIG. 11

ÇRIMI'S PREDICTED WAKE SHAPE



SKEW-HELICAL WAKE CONFIGURATION

DISTORTED WAKE

COMPARISON OF INITIAL WAKE GEOMETRY WITH THE CONFIGURATION AFTER PERIODICITY IS ESTABLISHED FOR μ = 0.14, λ = 0.00236, AND α_T = 2.3 DEGREES (REF. 13)

APPENDIX

Splitting the rings into polygons of N sides the equations of the various velocity components become:

$$V_{am_1}(t) = \sum_{i=0}^{N-1} \frac{\Gamma_2}{4\pi} \frac{(\cos\theta_i + \cos\theta_{i+1})}{hi} * \text{Z axis component} \left\{\frac{(r_1(t) - r_2(t))\wedge dr}{|r_1(t) - r_2(t)||dr|}\right\}$$

$$V_{r_1}(t) = \sum_{i=0}^{N-1} \frac{\Gamma_2}{4\pi} \frac{(\cos\theta_i + \cos\theta_{i+1})}{hi} * \text{radial component} \left\{\frac{(r_1(t) - r_2(t))\wedge dr}{|r_1(t) - r_2(t)||dr|}\right\}$$

$$V_{as_1}(t) = \sum_{i=1}^{N-2} \frac{\Gamma_1}{4\pi} \frac{(\cos\theta_i + \cos\theta_{i+1})}{hi}$$

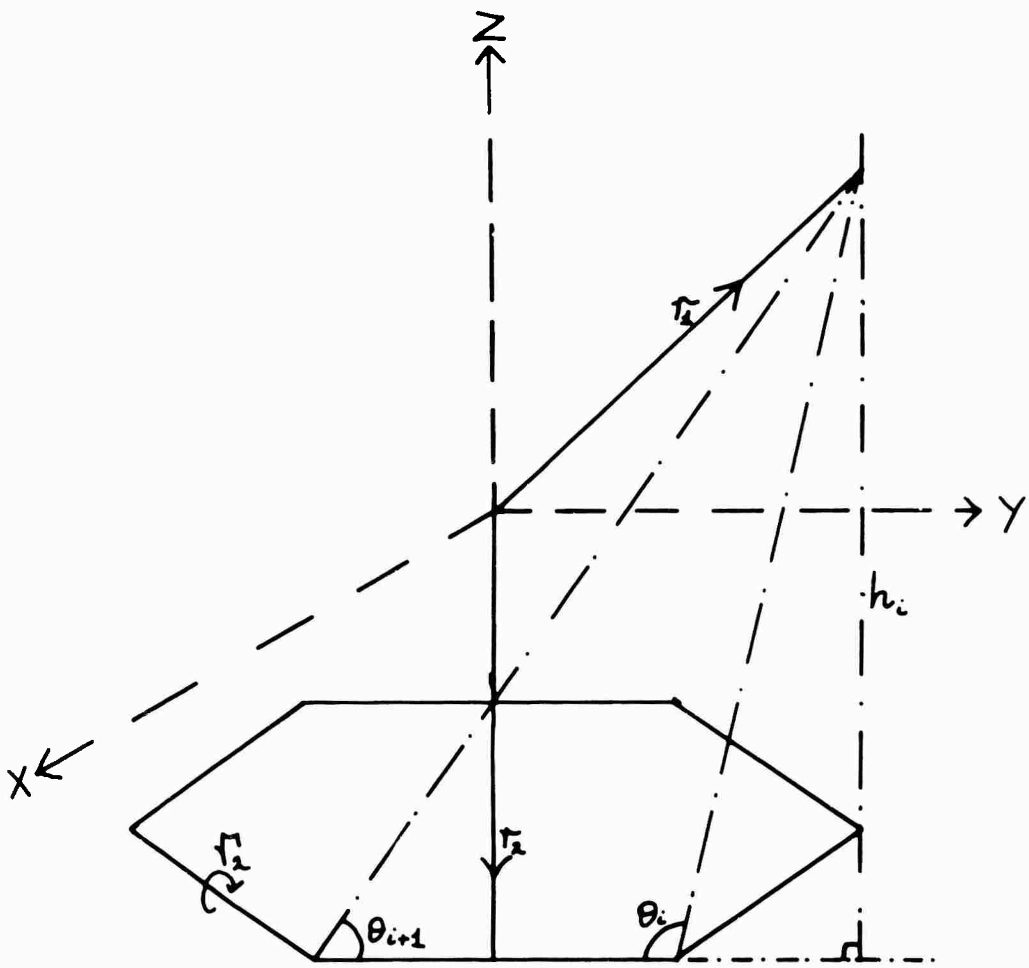
$$|r_1(t)|_{\text{radial component}} = \int_o^t V_{r_1}(t)\, dt + |r_1(o)|_{\text{radial component}}$$

$$|r_1(t)|_{\text{Z axis component}} = \int_o^t (V_{am_1}(t) + V_{as_1}(t))dt + |r_1(o)|_{\text{Z axis component}}$$

changing suffix to 2 gives motion of ring 2.

APPENDIX

Diagram of Representation of Ring by Polygon



To improve clarify ring 1 is not shown.